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THE PSYCHOLOGICAL REVIEW

THE DESCENT OF INSTINCT

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"The delusion is extraordinary by which we thus exalt language above nature:—making language the expositor of nature, instead of making nature the expositor of language" (Alexander Brian Johnson, *A Treatise on Language*).

The basic ideas underlying a concept of instinct probably are older than recorded history. At any rate they are clearly set forth in the Greek literature of 2,500 years ago. They have been controversial ideas and they remain so today. Nevertheless, the instinct concept has survived in almost complete absence of empirical validation. One aim of the present article is to analyze the reasons for the remarkable vitality of a concept which has stood without objective test for at least two millenia. A second objective is to evaluate the concept as it relates to a science of behavior.

ORIGINS IN PHILOSOPHY AND THEOLOGY

The concept of instinct evolved in relation to the broad problems of human destiny, of Man's place in nature, and his position in this world and the next. From the beginning, instinct has been defined and discussed in terms of its relation to reason and, less directly, to the human soul.

During the fourth century B.C. the Greek philosopher Heraclitus declared that there had been two types of creation. Men and gods were the products of rational creation, whereas irrational brutes comprised a separate category of living creatures. Heraclitus added the observation that only gods and men possess souls. The close relation between rational powers and possession of a soul has been reaffirmed time and again during the ensuing 2,500 years. Heraclitus did not advance the concept of instinct but he laid the groundwork for its development.

Stoic philosophers of the first century A.D. held that men and gods belong to one natural community, since they are rational beings. All animals were specifically excluded since they are not creatures of reason and even their most complex behavior takes place "without reflection," to use the words of Seneca. This stoical taxonomy was both flattering and convenient since, according to the tenets of this school, members of the natural community were forbidden to harm or enslave other members.

It is significant that neither Heraclitus nor the Stoics based their conclusions upon objective evidence. Their premises concerning the psychology of animals were not derived from empirical observation; they were demanded by assumption of the philosophical position that animals lack a rational soul.

Aristotle, who was more of an observer than a philosopher, was of a different mind. In *Historia Animalium* Man is placed at the top of Scala Na-

tura (directly above the Indian elephant), and is accorded superior intellectual powers, but none qualitatively distinct from those of other species.

In the thirteenth century Albertus Magnus composed *De Animalibus,* based chiefly upon the writings of Aristotle but modifying the Aristotelian position where necessary to conform to Scholastic theology. Albertus removed Man from the natural scale, holding that he is unique in possessing the gift of reason and an immortal soul. Animals, lacking reason, "are directed by their natural instinct and therefore cannot act freely."

St. Thomas Aquinas, student of Albertus, supported his teacher's distinction between men and animals. Animals possess only the sensitive soul described by Aristotle. The human embryo is similarly endowed, but the rational soul is divinely implanted in the fetus at some time before birth.[1] The behavior of man therefore depends upon reason, whereas all animals are governed by instinct. Like the Stoic philosophers, the Scholastics were unconcerned with factual evidence. Their emphasis upon instinctive control of animal behavior was dictated by a need of the theological system, and in this frame of reference instinct was a useful concept.

Roughly four centuries after the time of St. Thomas Aquinas, René Descartes and his followers aggressively restated the existence of a man-brute dichotomy. The bare facts of the Cartesian position are common knowledge, but for the purpose of the present argument it is important to ask why Descartes felt so strongly about the matter—felt compelled to hold up man as the Reasoner, at the same time insisting that all other living creatures are only flesh-and-blood machines. The explanation stands out in the following quotation:

"After the error of atheism, there is nothing that leads weak minds further astray from the paths of virtue than the idea that the minds of other animals resemble our own, and that therefore we have no greater right to future life than have gnats and ants" (René Descartes, *Passions of the Soul*).

From Albertus to Descartes the argument runs clear. The theological system posits a life after death. Hence the postulation of the soul. But mere possession of a soul is not enough. Each man must earn the right of his soul's salvation. This in turn depends upon reason, which man exercises in differentiating good from evil, behavior which is sinful from that which is not. An afterlife is man's unique prerogative; no animals share it. They have no souls and therefore no need to reason. But how are the complex and adaptive reactions of subhuman creatures to be explained if not by reason, foresight, volition? They are comfortably disposed of as products of instincts with which the Creator has endowed all dumb brutes.

That the thirteenth-century point of view persists today is shown by the following quotation:

> In animals there are only instincts, but not in man. As St. Thomas points out, there cannot be any deliberation in a subrational being (even though we may get the impression that there is). . . . Instincts in animals seem to operate according to the pattern of physical forces, where the stronger always prevails; for animals are utterly devoid of the freedom which characterizes man. . . . That is why when one studies human behavior one must rise above the purely animal pattern and concentrate upon those two faculties, intellect and will, which separate man from animal (Msgr. Fulton J. Sheen, *Peace of Soul*).

To summarize what has been said thus far, it appears that the descent of

[1] It is not irrelevant to point out that weighty disputation concerning the exact age at which the soul enters the fetus retarded the advancement of embryological knowledge during its seventeenth century beginnings.

the instinct concept can be traced from early philosophies which set man apart from the rest of the living world and sought for him some divine affinity. This was achieved by claiming for man alone the power of reason. By a process of elimination the behavior of animals was ascribed to their natural instincts. During the Middle Ages this dichotomous classification became a part of Church doctrine, with the result that possession of reason and of a soul were inextricably linked to the hope of eternal life. Prescientific concepts of instinct were not deduced from the facts of nature; they were necessitated by the demands of philosophical systems based upon supernatural conceptions of nature.

EARLY SCIENTIFIC USAGE

When biology emerged as a scientific discipline, there was a general tendency to adopt the prescientific point of view regarding instinct. Some exceptions occurred. For example, Erasmus Darwin's *Zoonomia* expressed the theory that all behavior is a product of experience, but this point of view was subsequently disavowed by the grandson of its sponsor. Charles Darwin made the concept of instinct one cornerstone of his theory of evolution by means of natural selection.

To bridge the gap of the Cartesian man-brute dichotomy, and thus to establish the evolution of mind as well as structure, Darwin and his disciples amassed two types of evidence. One type purported to prove the existence of human instincts; the other pertained to rational behavior in subhuman species. The idea of discontinuity in mental evolution was vigorously attacked, but the dichotomy between instinct and reason was never challenged.

The nineteenth-century literature on evolution shows plainly that the concept of instinctive behavior was accepted because it filled a need in the theoretical system, and not because its validity had been established by empirical test.

Contemporary psychologists such as Herbert Spencer were influenced by the evolutionary movement, and the idea of an instinctive basis for human psychology became popular. William James, in Volume II of his *Principles*, insisted that man has more instincts than any other mammal. McDougall's widely read *Social Psychology* listed human instincts of flight, repulsion, parental feeling, reproduction, self-abasement, etc. Woodworth, Thorndike, and other leaders agreed that much of human behavior is best understood as an expression of instinctive drives or needs.

One of the difficulties with such thinking is that it often leads to the nominal fallacy—the tendency to confuse naming with explaining. Some psychological writers were guilty of employing the instinct concept as an explanatory device, and the eventual result was a vigorous revolt against the use of instinct in any psychological theory.

THE ANTI-INSTINCT REVOLT

Dunlap's 1919 article, "Are there any instincts?" (5), was one opening gun in the battle, but the extreme protests came from the most radical Behaviorists as represented by Z. Y. Kuo, who wrote on the subject, "A psychology without heredity" (18). For a while the word "instinct" was anathema, but the revolt was abortive, and there were three principal reasons for its failure.

First, Kuo denied instinct but admitted the existence of unlearned "units of reaction." By this phrase he meant simple reflexes, but in using it he set up a dichotomy of learned and unlearned behavior which was fatal to his basic thesis. It merely shifted the debate to arguments as to the degree of complexity permissible in an unlearned re-

sponse, or the proportion of a complex pattern that was instinctive. The second error consisted essentially of a return to the position taken by Erasmus Darwin at the close of the eighteenth century. Having averred that the only unlearned reactions consist of a few simple reflexes, the opponents of the instinct doctrine invoked learning to explain all other behavior. This forced them into untenable positions such as that of maintaining that pecking behavior of the newly-hatched chick is a product of head movements made by the embryo in the shell, or that the neonatal infant's grasp reflex depends upon prenatal exercise of this response. The third loophole in the anti-instinct argument derived from a dualistic concept of the hereditary process. Admitting that genes can affect morphological characters, and simultaneously denying that heredity influences behavior, opponents of instinct were hoist by their own petard. If the physical machinery for behavior develops under genetic control, then the behavior it mediates can scarcely be regarded as independent of inheritance.

It is important to note that this war over instinct was fought more with words and inferential reasoning than with behavioral evidence. It is true that a few individuals actually observed the behavior of newborn children or of animals, but most of the battles of the campaign were fought from the armchair in the study rather than from the laboratory.

CURRENT THOUGHT IN PSYCHOLOGY

Although there are militant opponents of the instinct doctrine among present-day psychologists, it is undoubtedly correct to say that the concept of instincts as complex, unlearned patterns of behavior is generally accepted in clinical, social, and experimental psychology. Among experimentalists, Lashley suggested that instinctive behavior is unlearned and differs from reflexes in that instincts depend on "the pattern or organization of the stimulus," whereas reflexes are elicited by stimulation of localized groups of sensory endings (19).

Carmichael (3) expressed agreement with G. H. Parker's statement that human beings are "about nine-tenths inborn, and one-tenth acquired." Morgan (20) studied food-hoarding behavior in rats, and concluded, "since it comes out spontaneously without training, it is plainly instinctive." The following quotation reveals that some modern psychologists not only embrace the concept of instinctive behavior, but consider it a useful explanatory device.

"Of the theories of hoarding which have been advanced, the most reasonable one in terms of recent data is that the behavior is instinctive . . ." (28).

At least three serious criticisms can be leveled against current treatment of the problem of instinctive behavior. The first is that psychologists in general actually know very little about most of the behavior patterns which they confidently classify as instinctive. In his paper, "The experimental analysis of instinctive activities," Lashley mentions the following 15 examples:

1. Eating of Hydra by the Planarian, Microstoma.
2. Nest-building, cleaning of young and retrieving by the primiparous rat.
3. Restless running about of the mother rat deprived of her litter.
4. Homing of pigeons.
5. Web-weaving of spiders.
6. Migratory behavior of ashes.
7. Nest-building of birds, including several species.
8. Mating behavior of the female rat in estrus.
9. Dancing reactions of the honeybee returning to the hive laden with nectar.

10. Visual reactions of rats reared in darkness.

11. Responses of the sooty tern to her nest and young.

12. Reactions of the seagull to artificial and normal eggs.

13. Sexual behavior of the male rat.

14. Mating responses in insects.

15. Mating responses in domestic hens.

It is a safe guess that most American psychologists have never observed any of these patterns of behavior. At a conservative estimate, less than half of the reactions listed have been subjected to even preliminary study by psychologically trained investigators. The significance of this criticism lies partly in the fact that those psychologists who *have* worked in the area of "instinctive" behavior tend to be more critical of the instinct concept than are those who lack first-hand knowledge of the behavioral evidence.

Relevant to the criticism of unfamiliarity is the fact that the degree of assurance with which instincts are attributed to a given species is inversely related to the extent to which that species has been studied, particularly from the developmental point of view. Before the development of complex behavior in human infants had been carefully analyzed, it was, as we have seen, a common practice to describe many human instincts. Longitudinal studies of behavior have reduced the "unlearned" components to three or four simple responses not much more complex than reflexes (4).

The second criticism is that despite prevailing ignorance about the behavior which is called instinctive, there is strong pressure toward premature categorization of the as yet unanalyzed patterns of reaction. The history of biological taxonomy shows that the reliability of any classificatory system is a function of the validity of identification of individual specimens or even populations. Unless the systematist is thoroughly familiar with the characteristics of a given species, he cannot determine its proper relation to other groups. Similarly, until psychologists have carefully analyzed the salient characteristics of a given pattern of behavior, they cannot meaningfully classify or compare it with other patterns.

The third criticism of current treatment of instinctive behavior has to do with the classificatory scheme which is in use. When all criteria which supposedly differentiate instinctive from acquired responses are critically evaluated, the only one which seems universally applicable is that instincts are unlearned (21). This forces psychology to deal with a two-class system, and such systems are particularly unmanageable when one class is defined solely in negative terms, that is, in terms of the absence of certain characteristics that define the other class. It is logically indefensible to categorize any behavior as unlearned unless the characteristics of learned behavior have been thoroughly explored and are well known. Even the most optimistic "learning psychologist" would not claim that we have reached this point yet. At present, to prove that behavior is unlearned is equivalent to proving the null hypothesis.

Perhaps a more serious weakness in the present psychological handling of instinct lies in the assumption that a two-class system is adequate for the classification of complex behavior. The implication that all behavior must be determined by learning or by heredity, neither of which is more than partially understood, is entirely unjustified. The final form of any response is affected by a multiplicity of variables, only two of which are genetical and experiential factors. It is to the identification and analysis of all of these factors that psy-

chology should address itself. When this task is properly conceived and executed there will be no need nor reason for ambiguous concepts of instinctive behavior.

GENES AND BEHAVIOR

Experimental investigation of relationships between genetical constitution and behavior was exemplified by the pioneering studies of Yerkes (30), Tryon (27), and Heron (12). Interest in this area has recently increased, and a large number of investigations have been summarized by Hall (11) who anticipates a new interdisciplinary science of psychogenetics.

As Hall points out, the psychologist interested in examining gene-behavior relations has several approaches to choose from. He can compare the behavior of different inbred strains of animals currently available in the genetics laboratory. He can cross two strains and study the behavior of the hybrids. Selective breeding for particular behavioral traits is a well-established technique. The behavioral effects of induced mutations have as yet received very little attention but should be investigated.

It is known that selective breeding can alter the level of general activity (23), maze behavior (12), emotionality (9), and aggressiveness (17) in the laboratory rat. Inbred strains of mice differ from one another in temperature preference (13), aggressiveness (24), and strength of "exploratory drive" (26).

Various breeds of dogs exhibit pronounced differences in behavioral characteristics. Some are highly emotional, unstable and restless; whereas others are phlegmatic and relatively inactive (7). Special breeds have been created by selective mating to meet certain practical requirements. For example, some hunting dogs such as the foxhound are "open trailers." While following a fresh trail they vocalize in a characteristic fashion. Other dogs are "mute trailers." The F_1 hybrids of a cross between these types are always open trailers although the voice is often that of the mute trailing parent (29).

Inbreeding of domestic chickens for high egg production has produced behavioral deficiencies of various kinds. Although hens of some lines are excellent layers, they have almost totally lost the normal tendency to brood the eggs once they have been laid (15). The maternal behavior of sows of different inbred lines of swine is strikingly different. Females of one line are so aggressively protective of their young that they cannot be approached during the lactation period. Sows of a second genetical line possess such weak maternal interest that they frequently kill their litters by stepping or lying on the young (14).

Study of the effects of controlled breeding cast doubt upon the validity of any classificatory system which describes one type of behavior as genetically determined and another as experientially determined. For example, by manipulating the genotype it is possible to alter certain types of learning ability. As far as present evidence can show, the influence of genes on learning is as important as any genetical effect upon other behavior patterns commonly considered instinctive. There is no reason to assume that so-called instinctive reactions are more dependent upon heredity than noninstinctive responses; hence genetical determination is not a differentiating criterion.

THE MEANING OF GENETICAL DETERMINATION

Behavior which is known to vary with the genotype is often incorrectly de-

fined as "genetically determined" behavior. Although we can show a correlation between certain genes and particular behavior patterns, this is of course no proof of a causal relationship. Many other genes and nongenic factors are always involved in such correlations. This point is nicely illustrated by a series of experiments on audiogenic seizures in mice.

Susceptibility to fatal seizures is high in some inbred strains and low in others (10). When a high-incidence and low-incidence strain are crossed, the susceptibility of the F_1 generation is intermediate between those of the parental strains. So far the evidence strongly supports the conclusion that seizure incidence is genetically determined. However, the incidence of seizures can be altered without changing the genetic constitution.

This is accomplished by modifying the prenatal environment. Fertilized eggs recovered from the tubes or uterus of a female of one strain and introduced into the uterus of a female of a different strain will sometimes implant normally and produce viable young. This has been done using seizure-susceptible females as donors and seizure-resistant females as hosts. Under such conditions the genetical characteristics of the young are unaltered, but their susceptibility to fatal seizures is lower than that of their own genetic strain and higher than that of the "foster" mothers in whose uteri they developed (8).

Studies of this sort emphasize the important but often neglected fact that postnatal behavior is affected by factors acting upon the organism before birth. As Sontag has pointed out, this is true of human beings as well as lower species.

> Fetal environment may play a part in determining characteristics of the physiological behavior of any newborn infant. We are too often inclined to neglect this source of modification of physiological potential. Too frequently we think of the individual as beginning life only at birth. Yet because it is during the period of intrauterine life that most of the cells of the vital organs are actually formed, it is during this period that "environmental" factors such as nutrition, oxygen, mother's hormones, etc. are most important in modifying their characteristics (25, p. 482).

Another fundamental principle illustrated by the results of transplanting fertilized ova is that the uniformity of behavior which characterizes highly inbred strains of animals cannot be ascribed solely to homozygosity, but depends as well upon *minimal variability of the prenatal environment*. More broadly conceived, this principle implies that behavioral similarities and differences observable at birth are in part a product of intrauterine effects.

If forced to relinquish the criterion of genetical control, proponents of the instinct doctrine fall back upon the criterion of the unlearned nature of instinctive acts. Now learning is a process occurring through time, and can only be studied by longitudinal analysis. If instinctive acts are unlearned, their developmental history must differ in some significant fashion from that of a learned response.

THE ONTOGENY OF BEHAVIOR

No bit of behavior can ever be fully understood until its ontogenesis has been described. Had psychologists always recognized this fact, much of the fruitless debate about unlearned behavior could have been avoided.

Perhaps the most widely cited psychological experiment on development and instinctive behavior is that of Carmichael, who studied the swimming behavior of larval amphibians (2). He reared embryos in a solution which paralyzed the striped muscles but permitted normal growth. Animals that were thus prevented from practicing

the swimming response were nevertheless capable of normal swimming when placed in pure water. These findings are often offered as proof of the claim that swimming is instinctive. However, to demonstrate that practice is not essential for the appearance of a response is only the beginning of the analysis. This point is clearly illustrated by certain observations of insect behavior.

Gravid female moths, *Hyponomenta padella*, lay their eggs on the leaves of the hackberry plant and die shortly thereafter. The eggs hatch, the larvae eat the leaves and eventually become mature. Females of this new generation in turn select hackberry leaves on which to deposit their eggs. Another race of moths prefers apple leaves as an oviposition site. The difference between the two races has been perpetuated, generation after generation, for many centuries. It would appear to be the example par excellence of a genetically controlled behavior trait. But such an explanation is insufficient.

When eggs of the apple-preferring type are transferred to hackberry leaves, the larvae thrive on the new diet. Thirty per cent of the females developing from these larvae show a preference for hackberry leaves when it comes time for them to deposit their eggs (16).

The evidence is of course incomplete. Why only 30 per cent of the insects show a reversal of preference is not clear. It would be illuminating if the same experimental treatment could be repeated on several successive generations. Nevertheless it appears likely that the adult moth's choice of an oviposition site is influenced by the chemical composition of the food consumed during the larval period (6). If this interpretation is correct, the data illustrate the fact that a complex behavior pattern may be "unlearned" and still depend upon the individual's previous history.

Comparable examples can be found in the behavior of vertebrates. Stereotyped patterns of behavior appear with great regularity in successive generations under conditions in which practice plays no obvious role. Nonetheless such "species-specific" responses may be dependent upon previous experience of the organism.

The maternal behavior of primiparous female rats reared in isolation is indistinguishable from that of multiparous individuals. Animals with no maternal experience build nests before the first litter is born, clean the young, eat the placenta, and retrieve scattered young to the nest (1). However, pregnant rats that have been reared in cages containing nothing that can be picked up and transported do not build nests when material is made available. They simply heap their young in a pile in a corner of the cage. Other females that have been reared under conditions preventing them from licking and grooming their own bodies fail to clean their young at the time of parturition (22).

There are undoubtedly many adaptive responses which appear *de novo* at the biologically appropriate time in the absence of preceding practice, but the possibility remains that component parts of a complex pattern have in fact been perfected in different contexts. Whether or not this is the case can only be determined by exhaustive analysis of the ontogeny of the behavior under examination. Nonetheless, to define behavior as "unlearned" in the absence of such analysis is meaningless and misleading.

SUMMARY AND CONCLUSIONS

The concept of instinctive behavior seems to have originated in antiquity in connection with attempts to define a clear-cut difference between man and all other animals. Human behavior was

said to be governed by reasoning, and the behavior of animals to depend upon instinct. In his possession of the unique power of reason, man was elevated above all other creatures, and, incidentally, his use of them for his own purposes was thus morally justified.

Christian theologians adopted this point of view and averred that man was given the power of reason so that he could earn his own salvation. Similar privileges could not logically be accorded to lower animals. Therefore they were denied reason and their behavior was explained as a product of divinely implanted instincts. In both sacred and secular philosophies the concept of instinct served a practical purpose, although in no instance was there any attempt to validate it by examination of the empirical evidence.

The concept gained a central position in scientific thinking as a result of the Darwinian movement. Proponents of the evolutionary theory accepted uncritically the assumption that all behavior must be governed by instinct or by reasoning. Their aim was to demonstrate that animals can reason and that men possess instincts. The same dichotomy has persisted in experimental psychology. Attempts to eliminate the instinct concept were unsuccessful because those who made the attempt accepted the idea that all behavior is either acquired or inherited.

No such classification can ever be satisfactory. It rests upon exclusively negative definitions of one side of the dichotomy. It obscures the basic problems involved. It reflects an unnaturally narrow and naive conception of factors shaping behavior.

To remedy the present confused situation it is necessary first to refrain from premature classification of those kinds of behavior that are currently defined as unlearned. Until they have been systematically analyzed it will remain impossible to decide whether these numerous response patterns belong in one or a dozen different categories.

The analysis that is needed involves two types of approach. One rests upon determination of the relationships existing between genes and behavior. The other consists of studying the development of various behavior patterns in the individual, and determining the number and kinds of factors that normally control the final form of the response.

When these methods have been applied to the various types of behavior which today are called "instinctive," the concept of instinct will disappear, to be replaced by scientifically valid and useful explanations.

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CONCEPTUAL AND METHODOLOGICAL PROBLEMS IN INTERPERSONAL PERCEPTION [1]

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In studies of interpersonal perception, the process most often investigated has been given such names as "empathy," "social sensitivity," "accuracy of social perception," "insight," and "diagnostic competence." Despite variations in terminology and method, the studies have similar aims. Knowledge about interpersonal perception is intended to be significant for social psychology and personality theory, as well as for practical problems in leadership, marital relations, clinical work, and teaching. Many difficulties, however, prevent clear interpretation of the results so far obtained. We attempt here to point out major pitfalls, to evaluate research procedures commonly used or recently advocated, and to suggest better designs for studies in this area.

Need for Sharpened Conceptualization

In studies of empathy and its sister traits, the basic variable has been only hazily conceptualized. This difficulty characterizes early research in any area; "intelligence," "attitude," and "adjustment" have all suffered from inadequacies of conceptualization comparable to those afflicting empathy. Writers have inadequately specified just what they mean to measure, or to what extent the variable they study overlaps the variables in other investigations. Thus, one test of empathy finds out how accurately subjects predict the ratings acquaintances will give them. Another test of empathy requires that subjects estimate the musical preferences of the average factory worker. Not surprisingly, these tests correlate only .02 (12).

Implicit Assumption of Generality

One fundamental question concerns degree of generality. Is understanding of others a highly generalized trait, or is it a collection of response patterns which have only a surface similarity? From the failure of many writers to delimit their concept, one gets the impression that they expect some people to be consistently good judges of others, and some people to be consistently poor judges; that is, a rather general trait is assumed to exist. If a Judge does well in predicting what response Others 1, 2, and 3 will give to stimuli *a*, *b*, and *c*, some investigators evidently would expect him to do well in predicting the responses of others 4, 5, and 6 to stimuli *x*, *y*, and *z*. Only an expectation of this character would lead one to try predictions of musical tastes as a possible gauge of the effectiveness of a foreman, or to accept a test of ability to predict responses of office workers as a parallel form to a test of ability to predict responses of factory workers (15).

[1] One of the present writers has been engaged in empirical research on interpersonal perception, trying various testing techniques and methods of analysis (7, 8, 9, 10). The second writer has been investigating mathematical aspects of measures of interpersonal perception (2, 3).

A portion of this paper was presented by Gage as part of a symposium before the American Psychological Association, September, 1953. This work was supported in part by a research grant (M–650) from the Institute of Mental Health of the National Institutes of Health, Public Health Service.

A generalized trait such as "empathic ability" may profitably be used as a construct if changes in the individual's behavior from situation to situation are small compared to differences between individuals in the same situation. The fact that mental tests correlate positively makes "general mental ability" a useful concept (though we can also differentiate that concept into more specific subtraits). "Resistance to stress" appears to be much less general; the general trait must be replaced by more specific traits describing resistance to particular stresses.

So, perhaps, with accuracy in interpersonal perception. Accuracy in predicting another's responses in one situation does correlate with accuracy in predicting another set of responses (7, 21). But it is questionable whether this accuracy must be ascribed to an "empathic" process, or even to genuine understanding (4, 21). It is therefore critically important to know just when measurements of empathy in one situation justify generalization to other situations or to a construct transcending particular situations. Until a general "ability to understand others" is established, workers should proceed with great caution, and define in any theoretical statement or interpretation of results just what facet is being discussed.

Nonequivalence of Alternative Operational Definitions

One reason why empathy is inadequately conceptualized is that many investigators have been content to rely on a simple "operational definition." Having invented a face-valid technique to measure the adequacy with which one person could understand another, investigators have neglected to inquire into its meaning. Entranced by the beauty of their operations, they have cloaked these limbs with barely enough conceptual analysis to provide some scientific respectability for their reports.

Most recent studies of interpersonal perception require a Judge to predict the responses of an Other. The predictions are scored for accuracy against the actual responses or characteristics of the Other. The responses to be predicted and the experimental conditions for obtaining scores have varied greatly from one study to the next. To clarify what present tests deal with, and thereby indicate the possible subdivisions of the field, we draw attention to four components of the typical experimental design:

a. The *Judge* whom the experimenter is attempting to measure.
b. The *Other*(*s*) whom the Judge is asked to interpret.
c. The *Input*, or information concerning the Other which is available to the Judge.
d. The *Outtake*, i.e., the statements or predictions about the Other obtained from the Judge.

The experimenter may, for example, decide to ask (*a*) kindergarten teachers to observe (*b*) children, and, using (*c*) cues obtained during observation, to predict (*d*) the sociometric choices each pupil will make.

Understanding another person may be regarded as having two stages, which suggest two continua for classifying investigations. First, the Judge must take in information, perhaps by observing the Other, or perhaps by dealing with him over a period of time; the first continuum therefore deals with the *degree of acquaintance* of the Judge with the Other. Second, the Judge must interpret the information in order to arrive at predictive statements; the second continuum therefore deals with the *degree of extrapolation* or inference required between Input and Outtake.[2] An

[2] Meehl (17, pp. 68–71) has used a parallel distinction in identifying two possible applications of the phrase "clinical intuition": (*a*) to the situation in which the clinician cannot

experiment may be designed to make great demands on the intake process (little acquaintance) or the interpretative process (much extrapolation), or both, or neither. The extreme patterns are contrasted in Table 1.

This table makes it clear that understanding of other persons demands different things of the perceiver in different situations. If we ask a person questions about Others where he has had ample opportunity to learn the answers by experience (Pattern A), we are primarily measuring his knowledge. When we present him with questions which he cannot answer on the basis of past experience alone, we are measuring ability to acquire new knowledge. But different abilities are required, depending upon whether the difficulty he faces is that of gathering information (Pattern B), or of drawing inferences (Pattern C), or both (Pattern D). A Judge who performs well in one pattern might perform badly in another.

Classification of Studies According to Objects of Perception

It is also necessary to inquire just what "Others" are involved in any hypothesis; unless this is clearly delimited, it can only be assumed that the investigator is interested in a generalized ability to understand all other persons. Various studies have used quite different objects of perception, asking the Judge to predict:

a. how *persons in general* will behave;

b. how *a particular category of persons* deviates from the behavior of persons in general;

c. how *a particular group* deviates from the typical behavior of the particular category it belongs to;

d. how *an individual* deviates from the typical behavior of the particular group he belongs to;

e. how *an individual on a particular occasion* will deviate from his typical behavior.

We can show that each of these types of understanding may be useful. (*a*) General principles such as "All people have a need to be approved" are expectations which guide conduct. (*b*) The individual forms expectations about different categories of people: managers or labor leaders, for example. (*c*) The person discriminates within a category, to form expectations about a particular group he is associated with. An officer can make wise decisions about his men on the basis of a correct stereotype of enlisted men in general, but he can make even wiser decisions by taking the particular wishes of his own squadron into account. (*d*) One next comes down to describing the unique behavior of the individual Other, as in clinical diagnosis. (*e*) The final step, prediction of differences within the individual over occasions, is illustrated when a therapist decides that in certain sessions it is better to review than to introduce new interpretations.

Our five types of Others and four patterns point to 20 rather different ways in which an ability to understand Others may be defined. Though not all these combinations are equally significant, research plans and interpretations need to be specified in terms of some such concepts as these.

NEED FOR INTERPRETABLE SCORES

Comparing the Judge's predictions with the Other's actual behavior readily

be articulate about the *evidence* for his diagnosis; (*b*) to that in which the clinician cannot "show in what manner a particular hypothesis was *arrived at* from the stated evidence." These two aspects of intuition, namely, "evidence" and "manner of arriving at," seem to resemble our "acquaintance" and "extrapolation," respectively. With a high degree of acquaintance the judge would have a great deal of evidence, and a high degree of extrapolation would require what Meehl calls "the creative act of hypothesis-formation."

TABLE 1

FOUR TYPES OF STUDIES OF INTERPERSONAL PERCEPTION

	Pattern A	Pattern B	Pattern C	Pattern D
Judge-Other relationship	Much acquaintance	Little acquaintance	Much acquaintance	Little acquaintance
Input-Outtake relationship	Little extrapolation	Little extrapolation	Much extrapolation	Much extrapolation
Hypothesized process	When acquainted with an Other, the Judge has many opportunities to observe him; some Judges habitually take better advantage of these opportunities than do others, paying better attention to the Other and cumulating more information about him.	Encountering a stranger, the Judge has some opportunity to observe him; some Judges are better able than others to take advantage of this brief opportunity, hence cumulating more information.	When acquainted with an Other, the Judge has many opportunities to observe him; some Judges are better able than others to use the information thus acquired, together with some personality theory, to derive accurate statements about variables not observed directly.	Encountering a stranger, the Judge has some opportunity to observe him; some Judges are better able than others to use the information thus acquired, together with some personality theory, to derive accurate statements about variables not observed directly.
Illustration	Asking high school counselors to agree or disagree that "The majority of adolescents say they have conflict with their parents"	Having Judges interview strangers and then rate their command of English	Having husbands predict personality test responses of their wives	Asking clinicians to make predictions of scholastic success from projective tests
Quality represented in accuracy measure	Knowledge from past experience	Ability to observe	Ability to infer	Ability to observe and infer

yields an accuracy score, but this score is difficult to interpret because a large number of processes may be postulated to explain it. This problem may best be described if we treat for the moment the simple situation where the prediction and the Other's actual behavior are reported dichotomously, and the prediction may therefore be scored as right or wrong. The conclusions would be modified only in detail if the score were based on magnitude of errors.

Controlling Effects of Real Similarity

Consider the study where we have (*a*) the Judge's self-description, (*b*) the Other's self-description, and (*c*) the Judge's prediction of *b*. The responses to any item have three aspects:

RS (real similarity): agreement of *a* and *b*
AS (assumed similarity): agreement of *a* and *c*
ACC (accuracy): agreement of *b* and *c*

Only two of these three are independent relations. That is, when two of these relations are known, the third may be inferred. Thus, if AS and RS on an item are scored 1 (denoting agreement), ACC must be 1. Scores for the three relational variables are obtained by summing the values obtained on single items. Any score may be considered a resultant of the other two. What we regard our test as measuring therefore depends on how we choose to conceptualize the problem, as has been pointed out by Tagiuri, Blake, and Bruner (22). Empirical studies have reported relations between the scores—for example, that Judges more typical of a group have higher accuracy in judging members of that group. But this may result merely from the linkage represented in the operations defining the scores, for when AS is constant and greater than RS, ACC and RS are correlated. Such a conclusion is a logical necessity, not a psychological finding regarding any superior insight on the part of the more typical Judge.

There is evidence that AS—perceiving Others as similar *to oneself*—is highly general over items. A person tends to assume similarity to the same degree throughout a questionnaire, despite marked variety in the apparent content of the items (20). Moreover, the tendency is somewhat general over preferred Others; if the Judge's AS score toward one friend is high, it will probably be high when he predicts the behavior of another friend (20). AS relative to a liked person, however, does not predict whether AS will be high or low when the Judge predicts for a disliked person (20). There is some justification, then, for regarding differences in the Judge's AS from Other to Other as a reflection of the Judge's attitude toward the Others (6). The AS score is to some extent a reflection of the Judge's general attitude toward other persons. But probably the AS score is also influenced by the Judge's set while taking the test; for example, Lundy (16) found that Judges who acquired facts about Other while interacting under a pay-attention-to-yourself set displayed more AS than did Judges instructed so as to have a pay-attention-to-the-Other set.

While the ACC score has a simple operational definition, it clearly does not correspond directly to any simple construct or trait. One possible solution is to obtain separate estimates of more elemental component variables (cf. 2). In making such analyses, however, the investigator risks embracing new confusions as he divorces the old.

Hastorf and Bender (13) have proposed to subtract AS from ACC (which they call "raw empathy") to estimate "refined empathy." This proposal has serious weaknesses, which may be clari-

	$a \neq b$	$a = b$
	Real Dissimilarity (RD)	Real Similarity (RS)
$a = c$ Assumed Similarity (AS)	Unwarranted Assumed Similarity (UAS) $a = c \neq b$	Warranted Assumed Similarity (WAS) $a = b = c$
$a \neq c$ Assumed Dissimilarity (AD)	Warranted Assumed Dissimilarity (WAD) $a \neq b = c$	Unwarranted Assumed Dissimilarity (UAD) $a = b \neq c$

a = Judge's self-description; b = Other's self-description; c = Judge's prediction

FIG. 1. Possible combinations of assumed and real similarity of any dichotomous item.

fied by considering the possible configurations of responses on dichotomous items. If *a* is the Judge's self-description, *b* is the Other's self-description, and *c* is the Judge's prediction, the patterns shown in Fig. 1 are possible. (With more than two response alternatives per item, the "WAD" cell contains two distinct patterns—*b* and *c* alike, or *b* and *c* different—which would modify the following argument.)

Following Kelly and Fiske (**14**, p. 108), we see that the total ACC score over all items is WAS plus WAD. The AS score is WAS plus UAS. When a Judge is predicting an Other, we may regard the real similarity or real dissimilarity of this pair on any item as fixed independently of any social perception by the Judge. Now we may ask, within the real similarity (RS) items: If the Judge predicts correctly, is he accurate? Or does he assume similarity? Obviously, these questions are operationally identical. The count of such items represents "warranted assumed similarity," and there is no way to distinguish whether this represents the mental set to assume similarity or the ability to judge accurately. In the Bender-Hastorf correction procedure, subtracting AS from ACC, we find that AS on RS items cancels ACC on RS items. Thus the RS items do not enter the refined empathy score.

Among real dissimilarity (RD) items where he predicts correctly, we might ask: Does the Judge recognize the dissimilarity or does he assume dissimilarity? These questions are both reflected in the count of WAD items. The Bender-Hastorf refined empathy score is equal to WAD − UAS. Therefore, the refined empathy score has a perfect negative correlation with AS, when RS is held constant. Furthermore, it has higher range when Judge and Other are dissimilar. Clearly, Bender and Hastorf did not arrive at a measure of accuracy independent of AS and RS.

The four categories of items in Fig. 1 have two degrees of freedom after the total number of RS items for a Judge-Other pair is established. We can take out two scores, and would like those scores to be independent. What score will be most meaningful depends on the correlation between the various cells. No single cell yields a good score as it

stands, for the cell entry is influenced by RS.

One possibility is to employ the ratios WAS/RS and WAD/RD to summarize our information about the Judge. This procedure requires that enough items be used to keep the denominator large; otherwise, of course, the ratio becomes unreliable. In any case, however, the ratios for different Judges will be based on different items; this removes what may be an essential experimental control. The correlation between WAS/RS and WAD/RD should be determined. If these components are positively correlated, it follows that individual differences in prediction are more strongly determined by differences in ACC than by differences in AS tendency. The correlation will be negative if individual differences in prediction are more strongly influenced by AS than by ACC.

Distinguishing Stereotype and Differential Accuracy

The accuracy score may be divided in another manner (**2, 7**), yielding components which we may refer to as "stereotype accuracy" and "differential accuracy." The former refers to the individual's ability to predict the pooled responses of a given category of persons, whereas the latter refers to his ability to differentiate among individuals within the category.

Whatever score is used should reflect accuracy in predicting an Other at the intended level of specificity. If we are asking a Judge to predict the response of an individual to a personality inventory, we are probably interested in the fourth of our five types of Others, and want to measure "ability to predict how this individual deviates from the typical behavior of the particular group he belongs to." If accuracy is scored directly by comparing the prediction to the response of the individual, we are not distinguishing between two components which contribute to the Judge's success: his knowledge of the response that *any* individual in the subgroup is likely to give, and his knowledge of the way in which *this* individual deviates from the norm.

It is apparently desirable, when studying ability to predict at any one level, to obtain at least two scores: (*a*) ability to predict the typical behavior in the next-larger class to which Other belongs, and (*b*) ability to predict how Other deviates from the norm for this class. This would apply whether Other refers to an individual, a particular Army squadron, or some category such as "education majors."

There are three ways to measure differential and stereotype accuracy.

1. Where the Judge predicts the response of several Others, it is possible to determine the response of the average Other on each item, and the average of the Judge's predictions on that item. Thus we form an average profile of responses, and an average predicted profile. The distance between these two, possibly after removing differences in over-all average response, is a measure of stereotype accuracy (**2**). Here the stereotype that the Judge holds is inferred from his responses over many Others.

2. Where the Judge predicts for several Others, we can score each prediction for one Other against the responses of the remaining Others for whom the predictions were *not* intended (**4, 7**). Such "accidental" accuracy, when averaged over the unintended Others, reflects the understanding which is general over all members of the group of Others rather than specific to a particular Other. It provides a sort of "psychological chance" base line. Accuracy measured in this way is closely related to stereotype accuracy of the

first kind, but is also affected by the dispersions of predictions and self-descriptions (3, p. 472).

3. If we ask the Judge to indicate what proportion of a group will give a particular answer, or to mark the modal answer to be expected in a given group, his stereotype is expressed directly. This prediction can be compared with the actual responses of the group. It is quite possible that the stereotypes obtained by this direct method would not coincide with the stereotype obtained by the other two methods. Such a discrepancy between what might be called the Judge's "conscious" and "unconscious" concept of the group could be of considerable interest, and studies obtaining both measures on the same Judge are called for.

In interpreting a stereotype accuracy score, a relation analogous to the assumed similarity-real similarity interaction may be noted. A person who is similar to the group, and who predicts that the group will in general give responses similar to his own, will almost certainly have high stereotype accuracy. A person who is atypical will have low stereotype accuracy if he assumes that other people give the same responses he does. If he assumes that others are different from himself, he may have either high or low stereotype accuracy, depending upon what differences actually occur.

The responses obtained in studies of social perception may be scored in many ways. The various scores so obtained are likely to be experimentally linked. Observed correlations are then likely to be artifacts of the experimental design, rather than relations among the traits the scores are named after. This type of difficulty is illustrated in a recent study (19). The Judges provided predictions of the norm, i.e., of what the average Other would say. This is c in Fig. 1. Empathy was defined as agreement between c and the true norm ($\bar{b}$). Reality was defined as agreement between c and the average prediction of all Judges ($\bar{c}$). If $\bar{b}$ agrees with $\bar{c}$ on more than half the items (as it surely would unless the Judges as a group are unrealistic in the extreme), then it necessarily follows that the empathy ($\bar{b} - c$) scores of individuals will be positively correlated with reality ($\bar{c} - c$) scores. The actual correlation was .77. No meaning can be attached to this result. There is an empirical fact underlying it, which is adequately described by the degree of overlap between the true norm ($\bar{b}$) and the average predicted norm ($\bar{c}$). Correlations among scores should not be interpreted in terms of higher order psychological constructs unless the operational variables are free from artifactual linkage.

NEED FOR INDEPENDENT CRITERIA OF SOCIAL SKILL

Many investigators have hypothesized that empathy, or accuracy of social perception, is correlated with effectiveness in interpersonal relations, and positive correlations have been found in several studies. In some research designs (e.g., 5, 11, 18), however, a linkage between accuracy score and criterion gives rise to an artifactual correlation. The obtained correlation cannot be interpreted.

For example, when sixth-grade pupils ranked each other sociometrically, and also estimated what ratings they would receive, accuracy in estimation correlated .50 with sociometric acceptance (5). But, as the authors report, pupils tended to predict that they would be highly accepted. Those who are indeed highly accepted automatically obtain better accuracy scores, and this fact alone would account for the observed correlation.

Another investigator (18) asked employees to rate their department super-

visor. Then each supervisor predicted the ratings given by his department employees, and his predictive accuracy was scored. The correlation of accuracy with actual rating was .90. But we can expect a person to state that the group he is responsible for has good morale. This hypothesis alone is sufficient to account for the reported correlation, without introducing empathy or sensitivity as a construct. Suppose (to simplify) that the rating is on a 5-point scale, and that every supervisor predicts that his group will rate him 5 (very good). Now if each supervisor receives rating b, his accuracy score will be *5 minus b* (a low score representing high accuracy). Accuracy would obviously correlate *perfectly* with the actual rating. Random variations in the supervisors' predictions would lower the correlation, perhaps to the obtained value of .90. When findings can be explained parsimoniously as an artifact, investigators have the responsibility of making and reporting whatever analysis is necessary to preclude such possibilities.

Even if the predicted variate b is experimentally independent of the social effectiveness criterion X, X can have an artifactual correlation with predictive accuracy whenever b and X are correlated. In the study of supervisor's sensitivity (18), the rating by subordinates (b) correlated .86 with such a second criterion, executives' ratings of departmental production (X). This is a reasonable finding which we do not question. But if all supervisors made identical self-flattering predictions (c), their accuracy ($b - c$) in estimating workers' attitude would correlate .86 with this second criterion. The actual correlation was .82.

It is possible that accuracy in perceiving an Other improves one's effectiveness in dealing with that Other. But designs more subtle than that described above are required to establish such a relation. The response predicted by the Judge, on which the accuracy score is based, must not also be the effectiveness criterion of the study, nor may it be correlated with this criterion. One possible design would be to measure accuracy in estimating the *pattern* of Other's responses with elevation or social desirability eliminated (15, 23). A pupil's knowledge as to *which* Others will give him highest sociometric ratings should not be artifactually related to criteria of his popularity. The supervisor's or teacher's empathy might be assessed by determining if he knows in *which* respects his group is best satisfied. Another device is to use a "standard Other," requiring everyone whose accuracy is tested to make predictions for the same individual (21) or group (1). The investigator should take pains to test for the presence of artifacts by establishing whether the response-to-be-judged is uncorrelated with the criterion.

THE PROCESS OF SOCIAL PERCEPTION

The foregoing sections have emphasized the defects of recent conceptualizations and procedures in research on social perception. It would be unwise, however, to ignore the positive contributions stemming from this work: sharpened logical and psychological formulations and, particularly, insights concerning real and assumed similarity, stereotype and differential accuracy, and the dangers of artifactual relationships. These insights will apply to research on social perception in any kind of situation and with any kind of material—for example, even if the Judge is allowed to report freely on observations and interpretations of the real-life characteristics and behaviors of an Other.

Beyond this, some tentative substantive conclusions have begun to emerge, revealing what goes on when one per-

son perceives another. Is the Judge's perception actually determined in any one-to-one fashion by cues he receives from the Other? Or is the reaction to the Other more "global"? Results with the kinds of data that have been collected to this point strongly suggest that the latter alternative is closer to the truth. Various global dispositions of the Judge appear to account for much of the variance in accuracy scores.

Two dispositions of this kind can be identified. First, Judges seem to differ significantly in their over-all tendencies to react favorably or unfavorably toward Others, both before and after the Others are observed. The Judge's favorability seems to determine his predictions or perceptions in a way that goes well beyond any identifiable stimuli coming from the Other. Then, if the Judge likes the Other, he will predict favorable, socially acceptable self-descriptions by the Other on a questionnaire or rating scale. If the Other does indeed describe himself favorably, the Judge will be accurate. But this accuracy stems more from a fortuitous concomitance of general favorability sets than from any differentiated perception of the Other.

A second kind of disposition has been termed the Judge's "implicit personality theory" (2); this consists of "built-in" correlations that the Judge consciously or unconsciously imposes on the traits, characteristics, or behaviors of Others. If the Judge is disposed to see trait B whenever he sees trait A, he will be accurate whenever traits A and B actually occur together in a given Other, and inaccurate when they do not. Judges have been found to differ in the closeness and direction of the associations they implicitly assume between traits. These differences among Judges influence their predictions of Others' responses, again in a way that seems to go far beyond any identifiable stimuli coming from the Others.

Hence, in the bulk of research to date, social perception as measured is a process dominated far more by what the Judge brings to it than by what he takes in during it. His favorability toward the Other, before or after he observes the Other, and his implicit personality theory, formed by his experiences prior to his interaction with the Other, seem to determine his perceptions. Most of the research that leads to this conclusion has been in situations where degree of acquaintance has been low. But this conclusion also seems to follow from studies where clinicians have been the judges, using the richest data that their diagnostic methods can provide, and from studies where husbands and wives have judged each other.

Probably we should not be surprised at this conclusion. It has its analogies, of course, in visual and aural perception. The process of perception is so laden with affect, and so highly overlearned in the course of socialization, that the dominant role of global dispositions might well be expected.

Research to test the limits of these conclusions readily suggests itself. We can begin with situations where degree of acquaintance is close, and only a small degree of extrapolation is required. Under these conditions we can get a base line in which specific identifiable cues dominate the perception. Then we can increase the degree of extrapolation and decrease the degree of acquaintance. At what point will the Judge's implicit personality theory and over-all favorability-unfavorability begin to appear and then to dominate? This question will not be researchable, of course, until it is put in operationally defined terms. Nonetheless, it appears at present that we shall not need to go far to find the

perceiver rather than the stimulus determining the perception.

SUMMARY

This paper describes several conceptual and methodological problems in research on interpersonal perception and presents suggestions for dealing with them.

1. Sharpened conceptualizations of interpersonal perception processes are needed. It is often believed that accuracy in social perception constitutes a general trait. But accuracy has different operational definitions in different studies; this alone is sufficient to account for the contradictory evidence reported. Interpersonal perception makes different demands on the Judge, varying with the degree of acquaintance between the Judge and the Other, and with the degree of extrapolation required from Input to Outtake. Five types of Other are identified in various types of research, ranging from persons in general to intraindividual variations. Each of the definitions of the problem requires separate study.

2. In measuring accuracy of interpersonal perception, research workers should take account of the altered meaning of accuracy scores as real similarity of the Judge to the Other varies. Faults in previously suggested "corrections" are noted.

3. Distinguishing between stereotype and differential accuracy should also make for more meaningful results.

4. Many reported relationships between accuracy of interpersonal perception and effectiveness in interpersonal relationships are contaminated by artifacts. Methods of avoiding artifacts are suggested.

5. Social perception, in most research to date, appears to be more a global process than a one-to-one response to cues received from the Other.

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SOME EFFECTS OF MOTIVATIONAL PROCESSES ON COGNITION [1]

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Work on the influence of needs and attitudes on perception and other cognitive processes has proceeded with insufficient analysis of the problem. It is the purpose of the present paper to outline a number of possible effects of motivational processes on cognitive ones. The influences to be described are regarded neither as established nor as exhaustive, but are offered as hypotheses for future research.

Frequently it seems to be tacitly assumed that the mere presence of a need or attitude is sufficient to account for an observed effect. The task of research has become, therefore, one of relating the presence or absence of particular motivational conditions to quantitative variations in performance on cognitive tasks. The point of view to be presented here is, rather, that the finding of such a correlation only opens up a problem: that of understanding *how* motivation influences cognition (cf. **44**; also **6**, p. 139).

The point may be illustrated by reference to the question of the effect of attitudes on recall of controversial material. There is some evidence to indicate that we remember material we agree with better than apparently equivalent material with which we disagree (e.g., **14**, **32**). That we agree with certain points is no explanation of their superiority in recall. But if we take this finding as a point of departure, it is indeed possible to find plausible reasons why material we agree with should be favored in memory. These are, of course, only hypotheses which require independent testing. (*a*) It may be suggested that an attitude functions as context for related material (cf. **5**, p. 582). Presented data are understood in relation to the subject's existing attitudinal structure. Items which are in harmony with the attitude find their place in the structure in a simpler and more direct manner than does opposing material. There seems to be little doubt—although it needs to be demonstrated for material comparable to that used in the studies on recall of controversial material—that structured material is better recalled than unrelated items (**24**). The superior recall of items we agree with might follow, then, from the advantage of structured over unstructured data in memory. (*b*) Material we agree with may be better recalled because it is better understood than material which opposes our own attitude. Material which is understood is known to fare better in memory than that which we do not understand (**24**). A point we agree with is not, of course, better understood just *because* we agree with it, but because we have thought about it, have placed it in context, etc. Nor are data that confirm our attitudes always better understood; an insult may be very well understood. (*c*) Facts and arguments we agree with may have an advantage in memory over opposing ones when they are, to start with, more familiar. (*d*) Material we agree with may be received in a more friendly manner, while that with which we disagree may be rejected at the outset, regarded as nonsense, etc. The former condition

[1] This paper is part of a project undertaken when the writer was a Fellow of the John Simon Guggenheim Memorial Foundation.

is, very likely, the more favorable for recall. Other things being equal, material to which we have given attention is favored in memory. (*e*) Intention to recall, a factor known to be important for memory, may operate to favor items with which we agree.[2] We may try to remember certain points because we wish to refer to them again, to use them in argument, because they are flattering to us, etc.

Repression will not be discussed in the present connection because, even if it operates in the experiments under consideration, there would seem to be no way of knowing that it does. It would be necessary to have detailed knowledge of the repressions of individual subjects in order to guess what new data might be repressed by association with them. It is, in addition, highly unlikely that the experiments which deal with the recall of controversial material have set up the conditions necessary to produce repression.

While there is evidence that makes the above hypotheses seem plausible, it is also becoming recognized that there are conditions under which material which opposes our attitudes is favored in recall (cf. **2**, **4**). We may begin to define these conditions too. (*a*) We may remember something that disturbs us precisely because it does not fit into our schema. It might be that when an attitudinal structure is ripe for change, such disturbing, contradictory material is favored, while the organizing effects of attitudes enhance recall of confirming material at other times. (*b*) Points we disagree with may stand out, in contrast to repeated evidence for something we believe. It is known that such outstandingness can favor recall. The fact that outstanding items within a system are favored does not, of course, contradict the statement made earlier that structured data are more likely than unorganized ones to be well recalled. (*c*) We may remember something in order to refute it, to accuse somebody of it, in some way to cope with it, etc. Intention to recall, in the service of some other need, may also favor the opposing material.

Finally, certain attitudes may operate to produce about equal recall of material we agree with and of that with which we disagree—for example, an objective attitude, a desire to be fair, to hear both sides of the case, a wish to recall as much as possible so as to do well in the recall test, etc. (**2**).

It is clear from the variety of hypotheses presented above that it is necessary to have detailed information about a particular subject's understanding of, and attitude toward, the presented material if we are to attempt any predictions about the kind of material which will be favored in recall. It will also be clear that the really interesting problems about motivational factors in recall are not confined to the favoring of some particular kind of material, but concern also the qualitative dimensions of the individual subject's pattern of recall, involving the kinds of processes suggested here. It may even be that the individual's pattern of recall of personally relevant material is so distinctive that, in the hands of a skilled clinician, it may yield projective data which agree well with personal information derived from interview and projective methods.[3]

[2] We are not concerned here with the problem of how intention operates in memory. It should be pointed out, however, that the intention does not impose itself on recall, but seems to act by altering other known conditions of recall. The problem of the influence of intentions on cognitive processes may be of the same order as the problem here under consideration.

[3] I am indebted to Florence R. Miale for a preliminary demonstration of this kind.

Alper and Korchin (**4**, p. 35) make a similar suggestion: ". . . selective recall may well

The above hypotheses, it will be noted, seek the effects of attitudes on remembering *in terms of factors known to favor or inhibit recall.* They introduce no new determinants, but are largely statements of ways in which attitudes might alter the structural conditions of recall.

By contrast, much of the interest of current research in this field has been to find distortions of cognitive processes by needs and attitudes. Such distortions undoubtedly occur, although their frequency under normal conditions may be questioned. But it is here suggested that the influence of motivational processes on cognitive ones is not limited to distortions, and that such effects are not necessarily the ones most likely to give us an understanding of the processes involved. Rather, as has been pointed out elsewhere (22), a possibly more fruitful starting point for research would seem to be to look for changes by motivational processes *in accordance with* the nature of the material on which they act.[4] The present paper will seek to describe ways in which needs and attitudes may alter cognitive processes in a manner that does not violate the presented structure.

As is implied in the above remarks, it is maintained here that motivational processes may influence, but do not produce, cognitive organization. A vector (e.g., a need, expectation, intention, or attitude) or a trace system can influence a perceived form or another memory trace only if the latter already exists. The problem, as Wallach (52) has pointed out, is to determine the stage of organization to which a visual process must develop before central factors can influence it.[5]

To see motivational processes as operating through the presented structure is not, however, to deny the influence of needs and attitudes on cognition. In a recent paper, Postman (38) seems to reduce such influences to others which are not themselves motivational, viz., set and past experience. Certainly the latter processes have important influences on cognitive ones, and motivation *may* act on cognition through them. Furthermore, they seem to cut across a number of the other influences of needs and attitudes; but it will be maintained in the paragraphs that follow that they by no means fully account for the effects in question.

We are not concerned here with imagination, wishing, etc., which for present purposes may be considered to be very largely determined by motivational factors.[6] Rather, the present discussion is limited to some of the interactions which occur between motivational and structural determinants of cognition.

The following hypotheses are offered as possible modes of influence of needs and attitudes on cognitive processes. They may guide, but they do not, of

function here as it does in the so-called projective tests: the subject reacts to the material selectively in terms of his needs and tension-systems, the products of his recall being themselves projections of these needs and tension-systems." These authors fail, however, to follow up their suggestion with detailed comparisons of recall and personality data of individual subjects.

[4] The theoretical consequences of taking distortions as the paradigm of the influence of needs and attitudes on cognition have been shown elsewhere (22).

[5] The present writer fails to see how the assumption of *unbewusster Schluss* (e.g., 9) solves this problem. In order to initiate processes of inference or recall, perceptual data must already be organized. We cannot make inferences about an object until we can perceive it; the inferences thus do not account for, but presuppose, the perception.

[6] This is probably an oversimplification. It would be worth knowing to what extent even these processes are limited by our knowledge, experience and assumptions—both conscious and unconscious—about ourselves and about reality.

course, take the place of concrete analysis of particular problems. Some of these hypotheses are already to be found in the literature. Others seem to derive some support from existing studies, even though these were not specifically designed to test them. In the case of still other hypotheses, the task of testing them remains for the future.

1. A need or attitude may operate as a vector, pointing in one direction rather than another. It is sometimes possible, under the influence of a need or attitude, to find an item which would otherwise be unnoticed in the perceptual field. This is easy to demonstrate with camouflaged items. Likewise a recall vector may bring things to mind that do not occur spontaneously. (Of course the search refers to memory traces established in the past. The vector, to be effective, must have something to point to; aroused traces, supplying this, enable the vector to be effective. Here, as in other effects to be discussed below, the attitude or need operates in cooperation with the individual's past experience.)

It is likely that effects of pointing may be demonstrated with other vectors besides simple search vectors in perception and memory. It seems that, under the influence of a need or attitude, we are attuned to events to which we would not otherwise be sensitive. Under the pressure of an unresolved need, we find things to be angry or worried or hurt about.

This hypothesis seems to be essentially the same as earlier views of Bruner and Postman, and others (e.g., **9**, **42**) on selective sensitization to valued or needed aspects of the environment. In later writings these authors seem to regard this process as a function of expectancies or "hypotheses" only. Postman, for example, states that "There is little evidence for direct sensitizing effects of motivational conditions on perception" (**38**, p. 99). It is here suggested that this function needs to be re-examined with respect to motivational states.

Some experimental work on the relation of recognition thresholds to motivational states seems to be relevant. If these studies have indeed demonstrated a lowering of threshold for need-related material that cannot be accounted for by differential frequency or the operation of specific expectancies, a process of pointing may be operating. For example, Postman and Brown (**39**) have shown that experiences of success attune the individual to tachistoscopically presented goal words (e.g., "succeed"), while after failure the subject is relatively more sensitive to deprivation words (e.g., "obstacle," "failure"). A possible interpretation of this finding is that the persisting mood attunes the individual to material congruent with it. McClelland and Liberman (**34**) report comparable results for individuals differing in the strength of their need for achievement. For example, subjects (*Ss*) with a strong need for achievement recognized goal and instrumental words relating to achievement (e.g., "success," "achieve") faster than those whose need was weak. Two studies on the influence of hunger on perceptual sensitivity to need-related material seem also to be relevant. Lazarus, Yousem, and Arenberg (**31**) report that recognition thresholds for photographs of food objects declined with increasing hours of food deprivation (within limits set by the habitual eating cycle), and Wispé and Drambarean (**54**) found that need-related words were recognized more rapidly than neutral ones under conditions of food and water deprivation, but not when *Ss* were satisfied.

These experiments need to be repeated under conditions where possible sets for need-related items are not al-

lowed to develop during the experimental series. For example, the relative sensitivity to the *first* need-related item in the series should be studied (cf. 28). If the findings should be confirmed under these conditions, they would suggest the operation of the kind of sensitization to need-related material here described as pointing.

2. Closely related to pointing may be the organizing effects of needs and attitudes. Within certain limits we can voluntarily influence organizations in the perceptual field, grouping together items which would not spontaneously go together. Likewise in recall, interaction between a process and a trace which would not occur spontaneously may take place under the influence of a vector (27). In the case of other cognitive processes, the same organizing effects of needs suggest themselves. It is a fact of common observation that when a strong need or interest is aroused, the facts of experience organize themselves around it. When I am working on a scientific problem, for example, everything I read appears to bear on the issues with which I am concerned. It seems that under the influence of an aroused need we perceive similarities not otherwise noticeable. These vectors, I repeat, operate in conjunction with the individual's knowledge and previous experience. But here, as in the cases that follow, it is insufficient to refer to past experience alone.

3. The perception of other relations is likewise influenced by needs and attitudes. It has been suggested that under the influence of an aroused need, the psychological field may be restructured so that learning occurs (1). For example, an object previously seen as unrelated to a goal may come to be perceived as the means to attaining it, one event may acquire the meaning of a signal for another, etc.

4. An aroused need or attitude may act on cognition by selection among the various possibilities presented. This hypothesis has frequently appeared in the literature. It has not, however, customarily been separated from sensitization or pointing, as discussed above (e.g., 9, 42).

The following example, among the many possible, illustrates the selective effect of an attitude in cognition. If two individuals who hold opposed attitudes are presented with a given fact, they do not necessarily perceive the same fact, but each may select for it a different meaning out of several possibilities it presents. As Asch puts it (5, p. 584): "One can observe much adroitness in the manipulation of meanings in the interests of an undisturbed outlook." Selective effects of needs and attitudes are most familiar from the projective methods. In the Rorschach, for instance, a variety of interpretations may be given to a similarly perceived portion of a blot; consider, for example, the variety of actions attributed by different individuals to the animals in the side details in Card VIII. Examples of this kind could be multiplied.

The experimental literature provides comparable examples. Sanford (46, 47) and Levine, Chein, and Murphy (33) found that hungry subjects gave more food responses in the interpretation of incomplete or ambiguous pictures than did satisfied ones, and that the effect increased (within limits) as hunger increased. (The effect of the need cannot, however, be separated from possible selective effects of a food set, in the experiment of Levine, Chein, and Murphy.) There appears to be a selection of need-relevant interpretations from among the many meanings the ambiguous material could be given.

5. The need or attitude may supply context. Since the context may influence decisively the manner in which an item is experienced, it follows that a

given item may be differently viewed in accordance with the need or attitude aroused. This effect of attitudes was illustrated above in the case of a problem of memory. Needs may be expected to have comparable effects, since they function typically in need-object organizations or "sentiments."

6. One particular effect of the fact that needs and attitudes function as temporally extended organizations deserves special mention: memory traces relevant to these organizations may be aroused. Past experience has important effects on cognitive processes, of which the more relevant in the present connection may be such effects as the contributing of meanings, the establishing of norms or adaptation levels (20), and the rendering commonplace of some item of experience so that it is overlooked or its significance lessened in the cognitive field. Past experience likewise facilitates perception under conditions of reduced stimulation (37, 21, 23, 49, 50; but cf. also 17, 29). It probably acts also as a selective factor, favoring certain possibilities among those which are structurally given (e.g., 13, 53).

These effects of past experience have been demonstrated in cognitive situations having little bearing on subjects' needs and attitudes. The work on the role of past experience needs to be extended to other cognitive situations which have motivational relevance.

7. A need may arouse an expectation, which is known to have certain effects on cognitive processes. For example, Titchener long ago formulated a principle of prior entry. "The stimulus for which we are predisposed requires less time than a like stimulus, for which we are unprepared, to produce its full conscious effect" (51, p. 251). The sensitizing, organizing, and selective effects of sets have been mentioned above. Bruner has suggested that the function of expectancies "is to re-order the availability of traces" (7, p. 307; cf. previous hypothesis). A number of recent experiments have dealt with the effects of sets on cognitive processes and the conditions under which they operate (e.g., 41, 10, 11). It is clearly important to distinguish between direct effects of motivational processes on cognition and their indirect effects through the arousal of expectations, which frequently have similar consequences (cf. 38).

Postman and Crutchfield (43) have recently discussed the arousal of expectations by a state of need. These authors presented incomplete words for completion, varying the intensity of hunger of their *S*s and the degree of selective set for food responses. They found the effects of set in determining food responses to be larger than those of differing intensities of hunger; the relationship between the latter and frequency of food responses depended largely on *S*'s expectation for such responses. They conclude (p. 217): *"Intensity of need is one of the variables which modifies the operation of such general principles of cognition as selective 'set' within limits defined by the characteristics of the stimulus-materials."*

8. Needs or attitudes may make us unwilling to ask certain questions, discourage the desire to understand, keep us from considering relevant evidence or from seeing the relevance of presented evidence. They may narrow the mental field (1, 5), with significant consequences for cognition. Much current work has been concerned with such influences (e.g., the effects of prejudice, the clouding of judgment by strong emotion). These effects are not, at the outset, to be dismissed as entirely automatic evasions. Reasons are frequently used in these instances to give them at least the appearance of sense. The cognitive processes involved need to be understood.

9. Needs or attitudes, in a manner closely related to the last-mentioned ef-

fect, may cause us to overlook differences, to fail to make distinctions, just as in the perceptual field a great contrast may make us fail to see a lesser one. For example, several years ago the writer heard a debate on a perceptual problem held before a group which was bitterly opposed to the use of the experimental method in psychology. The audience seemed not to know that a debate was going on, but attacked both speakers alike for dissecting their human subject matter. They saw no important difference between the two positions presented. Common experience suggests similar phenomena in the realm of social and political attitudes. To a conservative person everything left of center may seem to be radical, while the more radical individual may regard everything right of center as reactionary. The range over which fine distinctions can be made appears to be shortened.

10. A possible effect of needs and attitudes is a specific disturbance of recognition and recall. Some evidence suggests that individuals who are shown, by independent methods, to differ in the extent to which given experimental material is disturbing to them, show corresponding differences in their recognition thresholds for such material (**15**, **16**, **30**). If these findings are confirmed, and if alternative explanations (e.g., differential readiness to report) can be eliminated, a disturbance of the recognition of certain kinds of threatening material suggests itself. Again, the facts of repression, insofar as this mechanism involves forgetting, raise a similar problem.

How might such a disturbance of recognition and recall operate? The phenomena of repression suggest that it is not a matter of destruction of memory traces; for repressed material to express itself in dreams, symptoms, and other disguises requires that the corresponding traces be intact. The disturbance seems, rather, to concern that interaction between process and trace, based on their similarity, which underlies recognition and is the first step in the process of recall by association. (For a discussion of the selective influence of similarity in recognition and recall, cf. Köhler, **26**, pp. 126 ff.)

It is very tentatively suggested, then, that a vector may operate to prevent that interaction between a present process and a memory trace which is necessary for recognition and recall.

11. Needs and attitudes may act on the physiognomic properties of experience. Consider, for example, the change which can sometimes be noted in a person's appearance for us with the change from acquaintance to friendship. That the change is not a matter of familiarity alone is suggested by the fact that such physiognomic changes seem to be much less pronounced when increasing acquaintance is not accompanied by friendship. It is true that the friend looks upon us with a more kindly eye than the mere acquaintance, may be happier and more relaxed with us, etc., so that some of the perceived change is accounted for by actual changes in his appearance. It is worth considering, however, whether changes in the person's physiognomic properties—changes which transcend differences in mood—may not be, in part, a function of the attitudes and sentiments we have for him.

It might be that, if Murray (**35**) has demonstrated a genuine influence of fear on judgments of maliciousness, it is of the nature of an effect on physiognomic properties. (Other interpretations, however, are possible.)

12. Needs or attitudes may animate, enliven, activate, or give outstanding position to relevant parts of the cognitive field. These effects, in turn, have certain consequences for cognition.[7] In

[7] This hypothesis does not depend upon a principle of "prior entry" (**9**, p. 96). It sounds like some of Bruner and Postman's statements about perceptual "accentuation"

a very real sense the significant person stands out in a group. The effects of outstanding position in perception, memory, and thinking are well known. As another example the possibility is suggested that Zeigarnik's main result —the favoring of incomplete over completed tasks in recall (**55**)—may be accounted for if it can be assumed that need tension may in some way make a trace more lively or more active and thus increase its availability. Gilchrist and Nesberg (**18**) have reported an experiment which suggests an interpretation in these terms. Hungry and thirsty subjects were asked to match the illuminance of just previously projected pictures of need-relevant objects. Increasing need gave rise to increasingly bright matches. In the case of thirsty subjects, the error dropped to its starting level immediately after drinking. If such a result should be confirmed, it could be viewed as an instance of the enlivening effect of need in immediate memory.

13. A strong need or interest leads us to exert efforts in its service. These may show themselves in increased efficiency in cognitive tasks, while a relaxation of effort reduces efficiency. In the case of excessive effort there may be a disruption of performance. A number of studies seem to permit interpretation in these terms rather than in terms of actual perceptual change.

Bruner and Postman (**8**) found that when *S*s were given electric shock during a task, they judged the size of a disc which figured in the task as accurately as control *S*s; but when the shock was removed, their size judgments increased. Since magnification in size is here equivalent to increased inaccuracy, it is suggested that this effect is not a matter of "posttension expansion" in perception, but of relaxation of efforts in a judging task. In another study (**40**) the same authors found that *S*s who had been harassed and badgered during an impossible perceptual task showed higher recognition thresholds in another perceptual situation than control *S*s, as well as premature and frequently nonsensical prerecognition hypotheses. It seems plausible to regard these findings as indications of disruption of test performance as a result of the actually reported excessive efforts of the experimental *S*s. Rosen (**45**), on the other hand, found that *S*s who were able to avoid an electric shock by correct perception had lower recognition thresholds for nonsense syllables than members of a control group who did not receive shock. Here it would seem that added, but not excessive, effort increased efficiency of test performance.

Other reports suggest comparable effects of attitudes in perception and memory. Allport and Kramer (**3**) found that anti-Semitic individuals were able to identify photographs of Jewish and non-Jewish faces more accurately than *S*s free of prejudice. The result is attributed to the greater importance of racial identity to prejudiced persons. Similarly Seeleman (**48**) found that different attitudes led to different degrees of effort and attention to an exposure series, with consequent differences of performance in a recognition test. Thus individuals with favorable attitudes toward the Negro correctly recognized more Negro photographs than did anti-Negro *S*s.

Such differences in performance are, of course, to be distinguished from actual cognitive change under the influence of a need or attitude.[8]

(e.g., **9**, p. 100). In practice, however, these authors have used accentuation only to mean increase in perceived size. As such, it comes under the heading of perceptual distortions, which are not being discussed here. In the present writer's opinion, accentuation of perceived size has, in any case, not been demonstrated as a function of value, with autochthonous factors properly controlled (cf. **2**, **12**, **19**, **25**, **36**).

[8] Postman (**38**) has discussed other motivational influences on performance in perceptual experiments, including the matter of selective

SUMMARY

This paper has been concerned with the question of how needs and attitudes influence cognitive processes. The attempt has been made to describe ways in which motivational processes alter cognitive ones in accordance with the nature of the material on which they act. It was suggested that needs and attitudes may act by pointing or sensitizing, organizing and reorganizing, selecting, supplying context, arousing relevant memory traces, arousing expectations, discouraging the desire to understand, obscuring differences, disturbing the recognition process, altering the physiognomic properties of experience, and animating or enlivening aspects of experience. In addition, strength of motivation may influence performance on perceptual or other cognitive tasks without producing actual cognitive change.

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ANXIETY, STIMULUS GENERALIZATION, AND DIFFERENTIAL CONDITIONING: A COMPARISON OF TWO THEORIES [1]

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Spence and Farber (4) and Spence and Beecroft (3) have recently reported a series of studies of differential eyelid conditioning of anxious and nonanxious subjects. In each experiment the positive CS (S_+) was a 500-cycle tone which was always followed after a brief interval by the unconditioned stimulus, an air puff. The negative or nonreinforced stimulus (S_-) was a 5000-cycle tone which was never followed by the air puff. In the four Spence and Farber experiments, differential conditioning procedures were employed throughout practice, in that S_+ was presented on half the trials and S_- on the other half. In the Spence and Beecroft experiment, 50 trials of simple conditioning (presentation of S_+ only) were followed by 50 trials of differential conditioning (half S_+ and half S_- trials). Anxious and nonanxious subjects were selected on the basis of the Taylor Anxiety Scale (4) or the forced-choice form of that scale (3).

In the present paper it is shown that the relative differentiation of the stimuli (represented by the difference in response strength of S_+ and S_- divided by the sum of the two response strengths) is about the same for anxious and nonanxious subjects. Two different methods of assessing response strengths are employed in the analysis. Response strength may be interpreted as "excitatory strength" (E) in accord with Spence's conditioning theory. In this situation E is related to the proportion of trials on which responses occur by the integral of the normal probability function (3, p. 400).

Using this measure of response strength the "E index of relative differentiation" is:

$$\frac{E_+ - E_-}{E_+ + E_-}$$

where E_+ is the excitatory strength of the positive CS, (S_+), and E_- is the excitatory strength of the nonreinforced negative stimulus, (S_-). Alternatively, response strength may be interpreted as "probability of response" (p) in accord with statistical theories of simple learning, represented here by the mathematical model of Bush and Mosteller (1). In this case, p is the proportion of trials on which responses occur.

The "p index of relative differentiation" is:

$$\frac{p_+ - p_-}{p_+ + p_-}$$

where p_+ is the probability of a response to S_+, and p_- is the probability of response to S_-.

Each of the five experiments under consideration (3, 4) involved two groups of subjects, comparable except that one group had scored high on the Anxiety Scale and the other group had scored low. We shall show that the average E index of relative differentiation of a group of anxious subjects is quite close to the average E index for the corresponding nonanxious group, and that the average p index for anxious subjects is also quite close to the average

[1] The authors are grateful to Dr. Kenneth W. Spence and Dr. Robert R. Bush, who read and commented on this paper. Dr. Spence also generously made available detailed results of the experiments.

TABLE 1

VALUES OF $(E_+ - E_-)/(E_+ + E_-)$ IN DIFFERENTIAL CONDITIONING IN FIVE EXPERIMENTS

Experiment		Anxious Group	Nonanxious Group	M diff	
Spence and Farber, Exp. I, Men	M	.328	.414	−.086	.55
	SD	.243	.433		
	N	10	10		
Spence and Farber, Exp. I, Women	M	.308	.234	.074	.44
	SD	.313	.313		
	N	8	8		
Spence and Farber, Exp. II, Men	M	.195	.208	−.013	.14
	SD	.260	.321		
	N	13	24		
Spence and Farber, Exp. II, Women	M	.132	.170	−.038	.66
	SD	.189	.291		
	N	40	25		
Spence and Beecroft	M	.296	.298	−.002	.01
	SD	.269	.293		
	N	15	24		

p index for corresponding nonanxious subjects. It will be shown that the Hull-Spence theory, in conjunction with special assumptions, is sufficient to predict that the E index of relative differentiation will be independent of anxiety. It will also be shown that the Bush-Mosteller theory, in conjunction with roughly the same assumptions, is sufficient to predict that the p index of relative differentiation will be independent of anxiety.

In general, the theoretical analysis shows that both theories are appropriate for analysis of these data. The two theories analyze the data in similar ways, though using different indicators of response strength.

ANALYSIS I

To determine the relation between anxiety and the E index of relative differentiation, the following computations are performed. For each subject E_+ and E_- are computed, based on the proportions of responses made to positive and negative stimuli. For each subject the E index, $(E_+ - E_-)/(E_+ + E_-)$, is computed.[2] These ratios are then averaged for each group, and the t's for mean differences between corresponding groups are calculated. The results of this analysis are shown in Table 1.

There is no indication that the E index of relative differentiation varies as a function of anxiety. The interpretation of this result depends upon a detailed theoretical analysis of the situation, based on the Hull-Spence theory.

In its simplest formulation, Spence's theory of conditioning holds that excitatory strength (E) is the product of habit strength (H) and drive level (D). Spence and his associates (3, 4) sup-

[2] For some subjects this ratio is indeterminate, since they made no responses to either the reinforced or the nonreinforced stimulus. These subjects were dropped from the analysis. In the case of the Spence and Farber experiments, data from Trials 61–100 are used. In the Spence and Beecroft experiment, Trials 51–100 are used. The data were made available by Dr. Spence.

pose that manifest anxiety is best represented in this theory by drive (D). If this assumption is correct, differences in anxiety will not affect the E index of relative differentiation. The basic assumption is that

$$E = H \times D. \qquad [1]$$

Let H_+ be the habit strength of the positive stimulus S_+, and let H_- be the habit strength of the negative stimulus S_-. Then

$$\frac{E_+ - E_-}{E_+ + E_-} = \frac{D(H_+ - H_-)}{D(H_+ + H_-)} = \frac{H_+ - H_-}{H_+ + H_-}. \qquad [2]$$

The result of Equation 2 merely shows that the E index is, by theory, unaffected by differences in D. One cannot argue backwards that since the E index is independent of anxiety, this shows that anxiety actually affects D. The reason is that, under certain circumstances, the E index of relative differentiation is also independent of H. For example, suppose that the habit strength of the negative stimulus, H_-, depends solely upon generalization from the habit strength of the positive CS or S_+. This will be the case if inhibitory effects are negligible. In these experiments inhibitory effects are probably very small. The intertrial interval of 15–25 seconds may be considered to be long enough to minimize inhibition effects, especially with the relatively small effort involved in a conditioned eyelid closure.

Supposing that H_- is generalized habit strength from H_+, we may apply Hull's equation for generalization (2, p. 199), which in the present notation is

$$H_- = H_+ \cdot e^{-j'd}$$

where e is a mathematical constant, j' is a parameter differing from individual to individual, and d is the difference between the stimuli (S_+ and S_-) in j.n.d. units. Since the form of the generalization gradient is somewhat in question, but probably depends upon both the individual (i) and the difference between stimuli (d), we shall use the more general function

$$H_- = H_+ \cdot f(i,d) \qquad [3]$$

where f is an unspecified function. It is easily shown that Equations 2 and 3 together imply that

$$\frac{E_+ - E_-}{E_+ + E_-} = \frac{1 - f(i,d)}{1 + f(i,d)}. \qquad [4]$$

Equation 4 shows that the E index of relative differentiation depends only on the generalization function $f(i,d)$ and not on H.

From this development it is possible to conclude that the E index will be the same for comparable anxious and nonanxious groups if and only if the generalization function, $f(i,d)$, is independent of anxiety. This theoretical outcome means that the discovered coincidence of E index values for anxious and nonanxious subjects shows that anxiety may affect either habit (H), or drive level (D), but that its effect cannot be attributed to the generalization function, $f(i,d)$. Another analysis is required to decide between D and H as correlates of anxiety.[3]

ANALYSIS II

To determine the relation between anxiety and the p index of relative differentiation, the following computations are performed. For each subject, p_+ and p_- are recorded. Then, for each subject, the p index, $(p_+ - p_-)/(p_+ + p_-)$, is computed. These ratios are averaged for each group, and the t's for mean differences between corresponding groups

[3] Anxiety must affect some part of the formula for E, since it is generally found that anxious subjects respond more than nonanxious ones in conditioning experiments (3, p. 399).

TABLE 2

VALUES OF $(p_+ - p_-)/(p_+ + p_-)$ IN DIFFERENTIAL CONDITIONING IN FIVE EXPERIMENTS

Experiment		Anxious Group	Nonanxious Group	M diff	t
Spence and Farber, Exp. I, Men	M	.523	.522	.001	.01
	SD	.239	.383		
	N	10	10		
Spence and Farber, Exp. I, Women	M	.414	.316	.098	.52
	SD	.351	.349		
	N	8	8		
Spence and Farber, Exp. II, Men	M	.298	.283	.015	.14
	SD	.291	.345		
	N	13	24		
Spence and Farber, Exp. II, Women	M	.209	.243	−.034	.44
	SD	.244	.367		
	N	40	25		
Spence and Beecroft	M	.443	.422	.021	.20
	SD	.308	.342		
	N	15	24		

are calculated. The results of this analysis are shown in Table 2.

There is no indication that the p index of relative differentiation varies as a function of anxiety. The interpretation of this result depends upon a detailed theoretical analysis of the situation, based on the Bush-Mosteller model.

The model for stimulus generalization and discrimination offered by Bush and Mosteller (1) contains no statement of the effect of anxiety on any of the functions or quantities involved. However, if special assumptions are introduced, it can be shown that the p index of relative differentiation is independent of anxiety.

The probability of response to any stimulus depends upon the effectiveness of reinforcement, upon inhibition or weakening of the response through exercise, and upon generalization from other stimuli which have been conditioned separately. Generalization, in turn, depends upon the original similarity of the two stimuli (eta, η), and upon the amount of "discrimination" between them which has occurred due to differential training. While the value of p_+ is influenced by reinforcement, the value of p_- is determined by inhibition and generalization, since response to S_- is never reinforced.

We first demonstrate that the effect of inhibition is small. According to the model, the probability of response to the positive stimulus, p_+, is raised by reinforcement and lowered by inhibition. The probability of response to the negative stimulus, p_-, is lowered by inhibition but is never increased by reinforcement. Therefore p_- will decrease if inhibition has a sizable effect. Since the data exhibit no systematic decrease in p_- during the test trials, the effect of inhibition may be assumed to be small.[4]

According to the Bush-Mosteller

[4] Conceivably, inhibition might tend to lower p_- but might be overcome by increased generalization from the positive stimulus. However, such increase in generalization effect can occur only if p_+ increases. The increase in p_+ observed during the test trials was small, and corresponding increases in p_- also occurred.

model, the degree of similarity of stimuli employed in a differential-learning experiment may decrease during training through the operation of the discrimination operator (1, pp. 419–420). If this were to occur, p_+ and p_- would diverge, since the positive effect of reinforcement on p_+ would have less and less tendency to raise p_-. In the test trials of the experiments employed for this analysis, no such divergence of p_+ and p_- is observed. Therefore, one may assume that the effect of the discrimination operator is small, and that the similarity of S_- to S_+ remains relatively constant throughout the test series.

It is concluded that the value of p_- is determined primarily by constant stimulus generalization from S_+ to S_-. The Bush-Mosteller formula for stimulus generalization, in the notation of the present paper, is

$$p_- = \eta p_+ \qquad [5]$$

in which case,

$$\frac{p_+ - p_-}{p_+ + p_-} = \frac{p_+ - \eta p_+}{p_+ + \eta p_+} = \frac{1 - \eta}{1 + \eta}. \qquad [6]$$

This indicates that the p index of relative differentiation depends solely upon similarity (η). Under these conditions it is clear that the p index of relative differentiation is independent of anxiety if and only if η is uncorrelated with anxiety. The discovered coincidence of p index values for anxious and nonanxious subjects shows that anxiety may affect any other part of the theory but its effect cannot be attributed to the similarity index η.[5]

DISCUSSION

This analysis points up the similarity and differences between the Hull-Spence theory and the Bush-Mosteller mathematical model in the study of stimulus generalization and discrimination. The theories differ in their index of response strength, in that Spence uses excitatory strength, E, whereas Bush and Mosteller use probability of response, p. However, both of these measures can be used to define relative differentiation indexes, and both indexes are found to be the same for anxious and nonanxious subjects. Each theory predicts this outcome when combined with the supporting assumptions (a) that inhibition effects are negligible, and (b) that the amount of generalization is proportional to the response strength of the reinforced or generalizing stimulus. Since the Bush-Mosteller model includes the additional assumption that similarity, or the proportion of response strength generalized, may decrease due to the operation of "discrimination," it is necessary in this case to assume that such discrimination has a negligible effect in the experiments under consideration.

The similarities of the theoretical analyses and the experimental outcomes mean that this study is of no service in choosing between the Hull-Spence and the Bush-Mosteller theories. Though the deductions from the two theories give slightly different results, the quantitative differences in this instance are so slight, relative to the variability of the data, that one theory is as successful as the other.

[5] Anxiety must, however, have some effect since p_+ averages higher for anxious than for nonanxious subjects.

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ASSOCIATION THEORY AND PERCEPTUAL LEARNING[1]

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The theoretical discussion of perceptual learning has received a welcome impetus from a recent critical analysis by Gibson and Gibson (6). These writers focus on two opposing interpretations of perceptual learning which they designate as the *enrichment theory* and the *specificity theory*. Enrichment theory is presented as the embodiment of associationistic thinking about perceptual learning, whereas specificity theory represents the hypothesis of psychophysical correspondence. The Gibsons' argument is directed at a refutation of the associationistic position in general and the enrichment theory in particular. At the same time, a strong plea is made for the hypothesis of psychophysical correspondence, as represented by the specificity theory. This paper will attempt to state the case for the associationistic position.

"Is perception a creative process or is it a discriminative process? Is learning a matter of enriching previously meagre sensations or is it a matter of differentiating previously vague impressions?" (6, p. 34). These are the questions to which the enrichment theory and the specificity theory are said to provide diametrically opposed answers. According to enrichment theory, perceptual learning results from the association of sensory elements with memories of past experiences. These memories may be conceived as images or as physiological traces. The essential point is that sensory elements produced by stimulation are assumed to be "enriched" by the arousal of such memories. As the fringe of associated memories grows, a constant sensory input gives rise to progressively more complex and more diversified experiences. This is Titchener's context theory or its modern equivalent. Such a view faces two major difficulties: (*a*) it perpetuates the distinction between sensation and perception, and hence inherits the thankless task of specifying the bare sensations to which memorial associations accrue; and (*b*) it implies that perception comes to be in *"decreasing correspondence with stimulation"* (6, p. 34). The richer the context which accrues to the sensory core, the further removed the percept is from the sensory data transmitted through the receptor system.

By contrast, the specificity theory holds that perceptual learning consists of the "progressive elaboration of qualities, features, and dimensions of variation" (6, p. 34). Such elaboration becomes possible because the organism responds to more and more variables of physical stimulation. Perceptual discrimination becomes more subtle and more refined because the stimulus variables with which it stands in psychophysical correspondence become more subtle and more refined. This view is said to avoid both of the difficulties upon which associationistic theory has foundered. It is (*a*) no longer necessary to distinguish between sensation and perception, for perception is from the beginning and is always a matter of psychophysical discrimination; and (*b*) whenever perceptual learning occurs, it means increasing rather than decreasing correspondence with the variables of stimulation. Note, however, that no

[1] Paper read before a joint session of Section I of the AAAS and the Western Psychological Association, Berkeley, December, 1954.

mechanism is proposed to account for the progressive changes in psychophysical relationships which constitute perceptual learning. At this point, the specificity theory is essentially the statement of an assumption or expectation, viz., that all perception follows a strict law of psychophysical correspondence. If perceptions change, there *must* be changes in the effective stimulus variables.

In developing our own discussion of the theory of perceptual learning, we shall find it useful to reaffirm the historical distinction between two facets of the associationistic position: the psychological and the physiological. Psychological associationism refers to the language in which the facts of learning are stated and the experimental problems are formulated. The conception of learning as consisting of linkages between ideas, or between stimuli and responses, exemplifies psychological associationism. Experimentally, the study of learning becomes the investigation of the conditions under which such linkages are established. Physiological associationism, on the other hand, refers to a class of hypotheses concerning the mechanisms which mediate the observed changes in experience or behavior. Thus, the hypotheses that learning depends on lowered synaptic resistances or on the formation of cell assemblies are examples of physiological associationism. We would expect to find the two kinds of associationism to be highly correlated in the thinking of psychological theorists. Physiological hypotheses which are associationistic in nature will necessarily be reflected in coordinate descriptions of behavior. It is possible, however, to argue for a psychological associationism without committing oneself to a specific physiological hypothesis. Such is precisely the position in which we shall find ourselves with respect to the problem of perceptual learning.

In their critique of the associationistic point of view, Gibson and Gibson have bracketed both its psychological and physiological implications. While stressing this distinction, we shall state our disagreement with the Gibsons' thesis on three counts: (*a*) The enrichment hypothesis discussed by them represents merely one historical stage in the associationistic approach to perceptual learning, and in some of its major aspects has long since been superseded by the formulations of behavior theory. Associationistic theory need not stand or fall with the historical version of the enrichment hypothesis. (*b*) Experimental methodology favors a formulation of the facts and problems of perceptual learning in the language of psychological associationism. (*c*) The specificity theory fails to generate testable hypotheses concerning the conditions and mechanisms of perceptual learning. We shall conclude that the specificity hypothesis has failed to challenge the associationistic position on either logical or empirical grounds.

Associationism and the Enrichment Hypothesis

The enrichment hypothesis, as exemplified by Titchener's context theory, represents the associationism of the structuralists and the introspectionists. The hypothesis had both a psychological and physiological component, and Titchener was careful to distinguish between them. Psychologically, a perception was regarded as a complex mental event which resolves into a number of sensations supplemented by images. Perceptual learning has occurred when an imaginal fringe has accrued to the sensory core, so that the complement of images "puts more in the perception than the sensory stimuli can account for" (12, p. 115). Both the nucleus and the fringe can be discovered by introspection. But, adds Titchener, "we

perceive more than is furnished us by sensations and images . . . . every perception is shaped and moulded by the action of nerve-forces which show themselves neither in sensation nor in image" (12, p. 115). The sensations and the images are the descriptive psychological data; the causative physiological factors behind them are dispositions in the central nervous system. In fact, in the case of well-practiced perceptions, the imaginal fringe may all but disappear and the meaning of a percept be carried by the neural dispositions alone. Thus, Titchener draws a sharp distinction between the givens of consciousness and the neural processes underlying them, between the "law of nervous action" and the "law of mental connection." He quotes with approval James's dictum: "Association, so far as the word stands for an *effect*, is between things *thought of*. . . . And so far as association stands for a *cause*, it is between *processes in the brain*—it is these which, by being associated in certain ways, determine what successive objects shall be thought" (9, p. 554). And Titchener sums up succinctly, *"The brain associates and meanings are associated"* (12, p. 149).

Thus, the structuralist position encompassed both a psychological and a physiological associationism. The arguments of Gibson and Gibson are directed against both these historical views. However, contemporary associationists have lost interest in Titchener's "law of mental connection" and have substituted for it a new psychological associationism. At the same time, many of them have continued the search for a "law of nervous action." We must evaluate the validity of the Gibsons' arguments not against the historical position of structuralism but against the psychological and physiological associationism of present behavior theory.

Psychological Associationism and the Criteria of Perceptual Learning

Associationists have long since left behind the introspective pursuit of bare sensations and accruing imaginal contexts. The timeworn arguments of the behaviorists and operationists which led to rejection of the introspective method need not be repeated here. The contemporary associationist deals neither with sensory cores nor with images. On the contrary, he would naturally begin by formulating the problem of perceptual learning in terms of stimuli and responses. The behavioral criterion of perceptual learning is a change in the nature of the responses evoked by a particular configuration or sequence of stimuli, where stimuli are defined in terms of physical operations. The two major classes of such responses with which we are concerned in experiments on perceptual learning with human *S*s are (*a*) identifying responses, such as naming or labeling; and (*b*) discriminative responses such as *same* or *different, larger* or *smaller, higher* or *lower*—in short, the types of responses which we obtain in conventional psychophysical experiments.[2] We assert that perceptual learning has taken place when the relative frequencies of such responses undergo significant changes under controlled conditions of practice.

Experimentally, then, perceptual learning is defined by changes in stimulus-response relationships under controlled conditions of practice. We see no alternative formulation, short of recourse to introspection or the vague language of phenomenology. Once the problem has been so formulated, the relevance of the variables governing stimulus-response association to perceptual learn-

[2] As I have suggested elsewhere (10), our conclusions concerning the parameters of perceptual discrimination gain in generality if they are based on observations using more than one class of discriminatory responses.

ing becomes clear. Examination of the literature shows that such principles as frequency, recency, effect, and associative interference are relevant to perceptual learning just as they are to verbal and motor learning (5, 10). The psychological associationism of a stimulus-response formulation serves to emphasize the continuity between perceptual learning and other types of learning, with respect to both experimental operations and functional relationships. Descriptively, perceptual learning *is* the attachment of new responses, or a change in the frequency of responses, to particular configurations or sequences of stimuli.

There are undoubtedly those who will say that insistence on a stimulus-response formulation robs the study of perception of its very core and, indeed, its fascination, for it looks away from the richness and subtlety of perceptual exeprience. We do not share this anxiety. There is no reason why all the valid facts of perception cannot eventually be translated into the language of stimulus-response relationships. In the meantime, the unpopular pedantry of insisting on stimulus-response formulations may serve a healthy purpose. In arguing for a stimulus-response formulation of perceptual experiments, Graham writes: ". . . it may be argued that it does not provide a description of perception. Certainly it does not describe *perception* if by that term is meant any area of knowledge that exists between the limits of sensory nerve impulses on the one hand and the language responses ('private experience') of the patient on the other. Such a usage, of course, deprives the term of any significance or specificity" (7, p. 69). We agree with Graham that a stimulus-response formulation helps to make perception an integral part of the study of behavior. By the same token, the problem of perceptual learning, formulated in terms of changing stimulus-response relationships, becomes part of the broader problem of associative learning.

The psychological associationism of stimulus and response avoids the two major difficulties which were raised in criticism of the psychological associationism of Titchener. (*a*) As Graham has pointed out, in terms of a stimulus-response formulation, "any presumed differences between 'sensory' and 'perceptual' research evaporate. Both types of research give rise to the same sort of stimulus-behavior function" (7, p. 69). (*b*) As for the question of whether learning results in increasing or decreasing psychophysical correspondence, the present formulation does not require us to give an a priori answer. We may adopt one of two positions. We can, as a matter of definition, restrict the term *learning* to those cases in which controlled practice results in improved discrimination. Preferably, however, we can leave the question open, and let the facts decide whether and under what conditions changes produced by training do or do not result in improved discrimination. The question of psychophysical correspondence cannot be decided by fiat except at the peril of begging the question.

The Problem of Associative Mechanisms

One may well object at this point that we have defended the associationistic position by means of a restatement in operational terms which avoids the problem on which the analysis of Gibson and Gibson has focused, viz., the problem of enrichment vs. differentiation. However, our insistence on a reformulation of the problem of perceptual learning in stimulus-response terms is an essential step in the development of the argument for two major reasons. (*a*) It was necessary to divorce once and for all the associationistic position

from the concepts and methods of introspectionism, modern or old-fashioned. (*b*) It was equally essential to emphasize the fact that a theory of perceptual learning, of whatever persuasion, cannot sidestep the fact that discriminative *responses*—words, movements, autonomic responses, etc.—change as a result of training. Gibson and Gibson appear to take it for granted that appropriate responses will be somehow attached to the differentiated perceptions developed in the course of learning. We wish to stress the fact that the changes in response are part and parcel of the problem of perceptual learning. The need to account for changes in response inevitably endows the problem of perceptual learning with an associative component.

The problem of mediating mechanisms, however, remains. One may, to be sure, rest content with the psychological associationism of stimulus and response, and pursue the specifications of the conditions of perceptual learning within this framework. I take it that this is essentially the position advocated by Graham. For those who have, like the present writer, failed to be convinced by the explanatory powers of current theories of mediation, the position has much to recommend for itself. It is true, nevertheless, that there is an old and continuing tradition of physiological associationism, and we must examine its logical status in the light of the criticisms advanced by Gibson and Gibson. Historically, the enrichment hypothesis does, indeed, represent a major strand in the tradition of physiological associationism. In evaluating the physiological facet of the enrichment hypothesis, we must stress again that a "law of nervous action" can stand as an independent hypothesis, divorced from assumed correlations with introspective content. There are modern formulations of the enrichment hypothesis which confirm its independence of introspective data. A good example is provided by Spence's schematic analysis of perception into a series of events, beginning with sense reception and followed by the redintegration of past sensory events, which in turn leads to anticipatory responses, manipulations, or verbal responses (11). In Spence's analysis, however, the associations aroused by the sensory events have the status of hypothetical constructs anchored to stimulus variables and observable responses, and do not depend for their validity on introspective or phenomenological evidence. It is possible to hold to a physiological enrichment hypothesis without implying an introspective distinction between sensation and perception. Nor does such a physiological associationism prejudge the question of increasing or decreasing psychophysical correspondence.

Physiological associationism does not necessarily take the form of an enrichment hypothesis. Consider Hebb's neurophysiological theory of perceptual learning. The fundamental assumptions of the theory are associationistic, but the process of perceptual learning is not described as the enrichment of invariant sensory events but rather in terms of progressive modifications of the central activities produced by stimulation. "According to the schema, the perception is constituted by a temporal sequence of activity in supra-sensory (or association-area) structures which owe their organization to change at the synapse" (8, p. 102). The changes at the synapse which constitute the physiological mechanism of learning govern the central transformations of sensory input.

The main point to be stressed concerning such physiological hypotheses is that one may assume an associative mechanism of perceptual learning without insisting that the events which are associated retain their identity or re-

main recoverable in perceptual analysis. This point is by no means new in the history of associationism. In contrasting the mental chemistry of John Stuart Mill with the mental compounding of James Mill, Boring writes: ". . . we never know about any element that *all* which we think ideally would enable us to predict the laws of the compound. We have always to study the compound directly, independently of its known or supposed elemental composition . . . even when we know the generative process, we cannot deduce the law of the resultant: those laws must be found in every case from direct experiment" (1, p. 230 f.). While Boring's remarks are directed specifically at the difference between two versions of psychological associationism, they apply to hypotheses about mediating mechanisms as well. Associationistic hypotheses about the mechanism of perceptual learning may be formulated without affirming that the associated elements must retain their identity in the resultant. What they must do is entail testable predictions about the resultant. Hebb and his associates have, indeed, been able to make specific predictions concerning the development of perceptual acuity which lend support to their assumptions concerning the generative process (8, p. 49 f.).

It must be recognized, however, that the body of facts supporting associationistic theories of the *mechanism* of perceptual learning remains small. For example, the perceptual constancies which are a mainstay of Helmholtzian empiricism continue to present a puzzle to physiological associationism or, for that matter, to any physiological theory of perception. It is true that the organism acts as though it had learned to make inferences about sizes, colors, and forms, but what the associative processes might be which underlie such inferential learning is far from clear. We can specify the cues which the organism appears to use in the attainment of the distal properties of objects (3), but we know little about how the organism comes to use certain stimuli as cues or about the mechanisms which make the utilization of these cues possible. It is easy to agree with Brunswik (4) that hypotheses about specific mediating mechanisms of perception may be premature at the present stage of development.

Much of the experimental work on perceptual learning has been done under conditions of impoverished stimulation. In such cases it is particularly plausible to conceive of a neural redintegrative process initiated by a fragmentary sensory input. However, the components of the redintegrative chain have never been sufficiently specified to constitute a testable theory of mediating mechanisms. This essential vagueness and invulnerability is shared by other physiological and neurological theories of perceptual learning, such as, for example, the gestalt theory of traces. The experimental facts have been all too ineffective in checking the elaboration of physiological fancies. However, to recognize that physiological associationism is as yet in a highly undeveloped state is very different from denying its possibility on a priori or logical grounds. We see no compelling reasons for such rejection.

Differentiation vs. Association

From the defense of associationism—psychological associationism now and physiological associationism as a possibility for the future—we turn to a reexamination of the specificity hypothesis. The critical difference between associationism and the specificity point of view appears to concern the degree of psychophysical correspondence assumed by the two positions. According to the

specificity hypothesis, perceptual learning consists of "responding to variables of stimulation not previously responded to," and hence, "perceptual development is always a matter of correspondence between stimulation and perception" (6, p. 34). By contrast, association theory is said to stress the enrichment of constant stimulus effects by the organism. It is our contention that the specificity position can be maintained only at the expense of avoiding or begging the question of learning.

What are the exact implications of the statement that perceptual learning consists of *responding* to variables of stimulation not previously responded to? If we take this statement at its face value, it would seem to assert that as a result of practice with certain stimuli new responses are associated with these stimuli. These responses, whatever their specific nature, have the function of mediating the differentiation of new qualities. We might, for example, conceive of such responses as receptor adjustments ensuring optimal exposure to the stimuli, such as focusing, scanning, etc. It is clear, however, that such an interpretation is not intended by the specificity theory, for perceptual learning is said to consist not of the attachment of responses to stimuli but of the elaboration of new qualities. Thus, contrasting the connoisseur of wines with a man whose untutored palate cannot tell the difference between Chianti and claret, Gibson and Gibson write: "What has the first man learned that the second man has not? . . . he has learned to taste and smell more of the qualities of wine, that is, he discriminates more of the variables of chemical stimulation" (6, p. 35). But if his learning is mediated neither by new receptor adjustments nor by centrally aroused associations, how are we to account for the acquisition of new differential responses? Surely we cannot assume that the stimuli have changed, for the stimuli are whatever they are. The fact that the organism has learned to discriminate more qualities is *the very fact that we need to explain.* The end result—the improvement in discrimination—cannot be accepted as a description of the learning process. It appears that the specificity hypothesis achieves its overriding emphasis on psychophysical correspondence by avoiding the critical problem of perceptual learning, viz., the nature of the processes which mediate the increased differentiation of the stimuli. Improvement in discrimination cannot be invoked to explain improvement in discrimination.

Inherent Limitations of the Specificity Hypothesis

Even though the specificity hypothesis may not qualify as a theory of perceptual *learning*, one may wish to uphold it as a descriptive generalization to the effect that after practice as well as before practice "the stimulus input contains within it everything that the percept has" (6, p. 34). Quite apart from the problem of mediating mechanisms, this generalization would assert that all the *results* of perceptual learning can be described adequately—and, indeed, exhaustively—as changes in psychophysical correspondence. Although such a generalization is logically tenable, its empirical adequacy is doubtful. The inherent limitations of the doctrine of psychophysical correspondence are brought out most clearly with reference to the perception of signs and symbols.

The perception of sign properties. Environmental events do not occur in complete independence of each other; the occurrence of one event implies, with some degree of probability, the occurrence of others. The existence of these sequential linkages makes it possible for

the organism to learn means-end relationships, to respond to objects as signs of absent significates (**13**, p. 135 f.). Tolman and Brunswik speak of the causal texture of the environment in which the organism is immersed and which is reflected in his perceptions and cognitions (**14**). A general theory of perceptual learning must account for the development of sign perception. But the discrimination of sign properties necessarily depends on commerce with environmental sequences. Hence, sign perception would appear to be, almost by definition, an associative phenomenon: an object can be perceived as a sign only by virtue of the fact that the organism has associated the sign object with the object signified. The fact of sign perception makes it difficult to accept the impassable barrier between perception and memory erected by the specificity theorists. It is, indeed, the analysis of sign perception which led Tolman to conclude that "the differences between perceptions and mnemonizations are probably, in actuality, always differences in degree only . . . there are probably no actual cases of pure perceptions—i.e., perceptions unaided by any memory—save in new-born organisms" (**13**, p. 139). The only alternative open to the specificity theorist is to assert that the facts of environmental linkage somehow become part of the *physical* stimulus pattern which constitutes the sign object! This would be tantamount to saying that the organism's past experiences somehow become translated into physical properties of stimuli, or at least that the organism comes to discover stimulus variables which carry the sign properties of the stimulus object in all their specific details. Every possible stimulus would have to have physical properties adequate for signifiying every possible significate.

The perception of symbols. Consideration of the perception of symbols similarly points to a breakdown of the specificity hypothesis. An important segment of the man-made perceptual world consists of symbols, of which words and numbers are the most familiar and frequent. Here are marks and sounds which the individual learns to perceive so that they carry meaning and significance. This is a case of perceptual learning par excellence, the environmental determination of which cannot be questioned. Little as we may know about the development of symbol perception, the very facts seem to demand an associationistic interpretation. Marks and sounds which are initially meaningless and undifferentiated come to be discriminated and identified by virtue of their appearance and usage in specific sequences and contexts. Again the specificity theory would have to maintain that all that is encompassed in the recognition or identification of a letter, a word, or a series of numbers or mathematical symbols is somehow carried by high-order stimulus variables. And, indeed, since any arbitrary mark or sound can be made into a symbol, all possible marks and sounds would have to have physical properties that can carry all possible connotations!

Thus the historical problem of meaning in perception raises serious difficulties for the specificity position. It is precisely here that stimulus-response analysis comes into its own. As Boring pointed out in discussing the behavioristic analysis of meaning, "Response is the context which gives the stimulus its meaning for the responding organism. One sees, therefore, that a theory of perception lies implicit in modern psychological positivism" (**2**, p. 18 f.). A theory of perceptual learning is equally implied. The organism has learned to perceive the meaning of a stimulus when it has learned to make the appropriate response.

SUMMARY

We have examined two contrasting approaches to the problem of perceptual learning—traditional associationism and the specificity hypothesis of Gibson and Gibson, which ascribes all perceptual learning to the increasing effectiveness of stimulus variables. The case for the associationistic position has been reaffirmed, and the adequacy of the specificity formulation has been questioned. The argument stresses three major points. (*a*) Perceptual learning is a problem of behavior change, and experimentally reduces to the study of stimulus-response associations. (*a*) Associationistic hypotheses about the mechanism of perceptual learning can be entertained without reference to introspective evidence, without commitment to a particular epistomological position and without prejudice to the problem of psychophysical correspondence. (c) The specificity hypothesis assumes, but does not account for, improvement in discrimination, and does not entail a testable theory of perceptual learning.

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WHAT IS LEARNED IN PERCEPTUAL LEARNING? A REPLY TO PROFESSOR POSTMAN

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Contemporary association theory, says Professor Postman (this Journal, pp. 438–446), formulates the problem of perceptual learning in terms of associations between stimuli and responses instead of associations between sensations and memory images. He seems to admit the theoretical difficulties in the way of an enrichment theory when the associations refer to phenomenal experience, but he implies that they are avoided when the associations refer to S-R connections. Here lies our main disagreement, for we do not believe that the difficulties can be so easily avoided. By reformulating the problem, can the associationist escape a reformulated difficulty? Professor Postman may not have to face the issue of whether perceptual learning is the enriching or the differentiating of experience, but he has to face another issue—whether such learning is a change in the attachment of responses to stimuli or an increase in the specificity of responses to stimuli.

Our critic should not suggest that the differentiation hypothesis we proposed is limited to conscious perception, or that it cannot be given a stimulus-response formulation. We believe it can be stated either in the language of phenomenology or in the language of behavior theory. The useful portions of both are intertranslatable. In the fable of the two winebibbers (perhaps from California and New York, respectively) we were at some pains to put the abstruse contrast between a tutored and an untutored palate in strict stimulus-response terms. And in the sample experiment we offered, concerning the development of an isolated identifying response, the procedure and the outcome were stated in these terms—namely, that there resulted an increased specificity of response to stimulation accompanied by an increase in the ability to respond differentially to the dimensional variables of the stimuli. An interesting thing about this outcome, however, was that it is also formulable in terms of what the subjects were aware of.

Professor Postman objects to the differentiation hypothesis for perceptual learning, presumably even in S-R terms, because no mechanism is proposed to account for the progressive change. It "begs the question" of learning; it does not explain *how* learning occurs (pp. 443–444). He must imply that the association hypothesis can account for the change and does explain how. But he himself, when discussing the difference between psychological and physiological associationism betrays a certain lack of confidence in the explanatory power of the concept. It is true that the history of psychology is full of "laws of association" having empirical validity, and that there are as yet no accepted "laws of differentiation" or "laws of stimulus-response specificity." But the age and respectability of the term "association" should not give it more explanatory power than it has. Our alternative hypothesis is not a theory but only the promise of a theory, and its explanatory value remains to be seen. It points to facts the explanation of which must be sought. It is concerned with the question of what is learned in perceptual learning, not how it occurs, or at least not as yet. "What is learned?" is the first question to ask,

we think; the question of the mechanism is secondary.

Even learning theorists who limit their research to animal behavior have been arguing the fundamental question of what learning is instead of how it occurs. Considering the performance of an animal before, during, and after practice, what *kind* of change in performance exists? Only if this is understood does the psychologist know what to try to explain. The "what is learned?" debate has so far been concerned with the response side of the stimulus-response formula. If a psychology of perceptual learning is to develop along with a psychology of motor learning the question must be faced on the stimulus side. This was the question explicitly formulated in our original paper. In what sense do we learn to perceive?

Professor Postman has his own idea of what perceptual learning is. He believes that it is "a change in the nature of responses evoked by . . . stimuli" (p. 440). It is also described on the same page as "changes in stimulus-response relationships under controlled conditions of practice" or, a little later, as "the attachment of new responses, or a change in the frequency of responses, to particular configurations or sequences of stimuli." The emphasis is wholly on the change in the responses. We argue that, where perceptual learning is concerned, it ought to be on the change in what the organism responds *to*. We suggested that perceptual learning consists of responding to variables of stimulation not previously responded to. We believe that the emphasis on change in the effective stimuli for responses will greatly increase the explanatory power of S-R theory. The stimulus can no longer be thought of as a bit of energy at a single receptor, true, but it never should have been so conceived in the first place. The organism is surrounded by energies of every sort and description. How they function as cues—that is, how certain variables and variations of this energy come to be specifically responded to—is the basic problem for perceptual learning. Just here is where psychophysics can help. Classical psychophysics has always been concerned with the correspondence between variables of energy and variables of response, although it was a serious mistake to suppose that the correspondence was innate or immutable.

The above quoted descriptions of perceptual learning, if taken as definitions, are not consistent with one another. It is said to be a *change*, either in the nature of responses, or in the frequency of responses, or in the stimulus-response relationships. But it is also said to be an *attachment* of responses to stimuli. This term reveals the strain of associationist thinking which we criticized and called the enrichment hypothesis. The changes in S-R relationships are not adequately described by speaking of the attaching, or connecting, or the forming of bonds between stimuli and responses. The inadequacy is evident if we inquire what is *new* in learning—new responses or new stimuli? Or is it merely the connections that are new? The change in S-R relationships that occurs with learning, we suggest, is one of progressive specification and abstraction. The organism discriminates and conceptualizes at the same time that he elaborates his repertory of responses. If both stimuli and responses are to be conceived as "molar" rather than "molecular," as Postman should be willing to admit, the change cannot be conceived as one of attachment between entities.

The main difficulty in the way of the traditional enrichment theory is its implication that learning involves a decreasing psychophysical correspondence between perception and stimulation.

But the associationism of stimulus and response, Postman says, avoids this difficulty. Does it? We suggest that it only refuses to face the difficulty. Postman himself pleads that he does not wish to prejudge the question of increasing or decreasing correspondence between perception and stimulation. He suggests that we let the facts decide whether practice does or does not result in improved discrimination. He will assert that perceptual learning has taken place when the *relative frequencies* of judgments of *same* or *different, larger* or *smaller, higher* or *lower* have changed (p. 440). He seems not to care whether the judgments are correct or incorrect. The student of perception, however, has to face the problem of veridical judgments. He has to count or compute *errors*. The experimental methods for studying discrimination require it. Let us try to push Postman to the wall. Does he really wish to assert that a progressive decrease in discriminative accuracy (increase in variable error or constant error) should be considered learning?

We come finally to the problem of signs and symbols, and the perception of meaning. Our specificity hypothesis cannot account for such facts, it is said, and yet this is required of a theory of perceptual learning. ". . . sign-perception would appear to be, almost by definition, an associative phenomenon: an object can be perceived as a sign only by virtue of the fact that the organism has associated the sign object with the object specified" (p. 445). This sounds very convincing. It is true that the theory of association comes into its own when treating signs, signals, symbols, indicators, cues, clues, surrogates, or substitute stimuli. The relation between a word and its referent has fascinated thinkers for centuries. Perhaps this is why the theory of association has lasted for centuries. It should be noted, however, that in the quotation above Postman falls back on the kind of association he has rejected—that between objects of experience. To have made the same point in terms of association between stimulus and response would have been much more difficult. The plausibility lent to classical association theory by its success with signs and symbols is not inherited intact by an S-R association theory.

The relation between a word and its referent is the best example of an association. It is arbitrary; it seems wholly unlike the relation between a pitch and a tone, or a wave length and a hue. Is it then the relation which should be postulated to exist between a stimulus and a response, as Postman does? In the case of perceptual responses, this seems to violate some of the facts.

A stimulus-response theory with emphasis not on association but on specificity of responses and discriminative behavior could be extended to explain meaning, signs, and symbols even though it has not yet been formally attempted. We do not agree that the specificity hypothesis breaks down when it comes to the problem of human reactions to man-made sources of stimulation. Just because they are man-made, the problem of perception is one stage more complicated with such objects than it is with objects of the natural environment, and the problem of perceptual learning is equally more complicated. Nevertheless the basic tenet of the specificity approach still holds: men learn to perceive symbols by a process which involves an increase in the specificity of their responses to physically different symbols. Symbols, like natural objects, must be differentiated or identified in order to be carriers of meaning. They come in sets, not singly. And it is quite possible that the meaning of a symbol, in the mathematico-logical sense, is given by its univocality within

the set. This seems to be the assumption which has proved so fruitful in information theory. The meaning of a symbol in the psychological sense may be given by the univocality of the response to the stimulus. Note the difference between this formula and Postman's. He says the organism has perceived the meaning of a stimulus "when it has learned to make the appropriate response" (p. 445). We would say *when it has learned to identify the stimulus relative to all possible stimuli.*

A theory of perceptual learning which takes the line suggested would unquestionably throw a great burden on the stimulus. The specificity hypothesis for perceptual learning depends on the validity of a psychophysical approach to perception itself. The organism must be ultimately capable of response to extremely high-order variables of stimulation, including those of temporal succession, if meaning is to be explained in this way. Professor Postman has pointed out what seem to be implausible complexities of such stimulation. Others could be cited which seem equally absurd on first consideration. The only possible answer to this argument is to ask for time to test the absurdities experimentally. Meanwhile the possibility of a new empiricism based on discrimination at least should be considered along with the old empiricism based on association.

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THE STUDY OF ALTERNATIVE RESPONSES BY MEANS OF THE CORRELATION COEFFICIENT

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In studies of the meaning of variables, the correlation coefficient appears to be used in two kinds of situations. Perhaps the more general use is the inferential one where the meaning which may be ascribed to a given variable tends to accrue from the nature of its correlations with other variables. Correlation coefficients may also be used deductively, i.e., hypotheses concerning the meaning of a variable may be tested by correlating the variable in question with other variables which have an established place in our conceptual scheme (either by virtue of their theoretical position or of their familiarity).

A danger inheres in using correlation coefficients as a device for examining the meaning of behavioral variables; we may in certain instances be led to fallacious conclusions. This possibility arises because of certain peculiarities of behavior. Specifically, in many kinds of situations it is possible that various alternative responses may be equally appropriate and may have the same general behavioral (or adaptive) significance. Nevertheless, because of their alternative nature, behaviorally equivalent responses may not always be conspicuously correlated with one another. For example, there are situations, particularly those which have a social relevance, where one of several functionally equivalent responses is sufficient, and if an individual offered more than one such response, it could be regarded as uneconomical, illogical, or unconventional.

It is also possible that any one of several responses may change the environment of an individual in such a way that in a given situation two functionally equivalent alternative responses are impossible for him. For example, at any one time one may rid oneself of old newspapers by burning them or by having the Salvation Army take them, but one cannot do both. So it is obvious that burning newspapers and calling the Salvation Army will not be positively correlated with each other as responses, although in some important respects they have a functional equivalence.

Currently, *drive* is one of the most promising behavioral concepts. It is probable, however, that correlations between responses cannot always be safely used either to infer the nature of the common drive underlying several alternative responses or to test hypotheses that several alternative responses may have a common drive. This is because drive is conceived in terms of a response-producing, excitatory state which is terminated by any one of several functionally or adaptively appropriate responses. It is commonly observed in the study of neurotic individuals that several different symptoms may serve the neurotic requirements (e.g., the unrecognized drives of the patient) equally well, so that during the course of therapy the disappearance of one symptomatic expression may be followed by the appearance of another without any diminution of the neurotic requirements of the patient.

A description of one aspect of the general problem created by the possibility of "alternative" responses has been provided by Else Frenkel-Brunswik (1). For her subjects an over-all

rating of the strength of the aggression drive had been prepared. In addition, observations were made with respect to two possible modes whereby the subjects might express their aggression. One of the observed modes was called "exuberance," the other "irritability." Both modes of expression were positively correlated with the judged strength of the general aggression drive; nevertheless, the correlation between the observed manifestations, "exuberance" and "irritability," was negative.

The fact that certain alternative responses serve the same function is often inferable from the similarity of the contexts in which the alternative responses occur. In some instances we are so familiar with the context that there is little danger of our making an inappropriate use of a correlation procedure in order to examine the functional similarity of the responses. From the context of numerous experiences, we know that in general the reaction of a hungry person toward chicken is similar to his reaction toward steak; nevertheless, at a meal where hungry persons might eat both chicken and steak, we would not expect to demonstrate this similarity by computing a correlation between the consumption of chicken and the consumption of steak. As a matter of fact, the correlation could be negative if some of the persons showed a marked preference for either chicken or steak.

In general, wherever our inferences or deductions must depend solely on the appearance of conspicuous correlation between isolated pairs of responses, the sound development of our science faces a possible hazard. It would appear that our inferential studies that employ a simple direct correlation between two variables may fail to draw to our attention certain important functional relationships. Similarily, if we are employing a deductive procedure and wish to test hypotheses concerning the functional similarity of two variables, we may fail to find evidence which would enable us to refute the null hypothesis, when in truth the null hypothesis (the hypothesis of no functional similarity) is indefensible. It would appear that our familiarity with the context in which these responses appear is our only defense against the hazard of alternative responses; if similarity of total context leads us to believe that two responses serve a similar function, we have good reason for challenging the empirical evidence of a negligible correlation coefficient between the two responses.

Similar Contexts as a Basis for Inferring Functional Similarity of Responses

It is apparent, therefore, that we should construct an analytical model which would protect us from a failure to show intrinsic functional relationships which are important to our conceptualizations, but which are not manifested by restricted, single correlations between pairs of alternative responses. Perhaps we can construct an analytical model that would make use of our knowledge of context in much the same way that a clinician makes use of context when he makes judgments concerning the functional similarity of various alternative facets of neurotic behavior. If the functional similarity of two alternative responses may be satisfactorily inferred from the fact that they occur in similar total contexts, perhaps a suitably descriptive correlation coefficient between them could be derived from a consideration of numerous aspects of the behavioral context in which they occur. One way of making a statistical study of the similarity of the contexts in which two responses occur involves determining the correlation coefficients between each of them and a large number of other responses and situations. If the second member of the pair of responses

shows the same pattern of correlation with the same large number of other responses as the first member of the pair, we could suspect that the two responses in question have a functional similarity, and perhaps we could reason that the more highly similar their total patterns of correlation with other variables, the more highly correlated we should consider their functional roles.

One way of expressing this idea would be to consider a context of k responses. Each of the two paired responses in question could be correlated with each of the k responses comprising our context. In this way two columns or sets of correlations could be listed. These sets of correlations would be paired, in the sense that each of the context variables would have a correlation with both the first response and the second response. Accordingly, the respective context correlations for Response 1 could be paired with the respective context correlations for Response 2, and the derived correlation between paired correlations could be computed. The correlation between the columns of correlations (cf. Spearman's intercolumnar correlations [2]) would be a correlation between the two responses which expresses the similarity of these two responses with respect to a context of k responses. As a result of such an analysis, we might find that the *derived* or intercolumnar correlation (based on context) was much larger than the original direct correlation (independent of context). Consequently, we might be inclined to suppose that from the standpoint of the functional similarity of the responses, the derived correlation had greater descriptive value than the original correlation. It is obvious, however, that such differences between derived and original correlations could be due to sampling errors in our original correlation coefficients, including those that are employed in computing the derived correlations. Accordingly, we must give some consideration to the conditions that we should expect to characterize a valid use of the principle of derived correlations as a means of studying the behavioral similarity of alternative responses.

An Illustration

Let us refer our discussion to a situation where alternative responses are possible. Perhaps such a situation would exist if we were to ask a group of individuals a series of questions about their own behavior; most people are characterized by certain common consistencies in their behavior that they themselves may not directly reveal, and may tend to conceal or deny by offering one of several socially sanctioned alternative descriptions of themselves. (It is possible that the sanctioned self-descriptions are in part a function of the implicit, aggregate characteristics of the individual.)

We secured the responses of one hundred five-year-old children to a series of questions concerning their behavior in specified situations. The questions were presented to each child individually, and the responses were classified on the basis of a limited set of probable responses that had been established in pretesting. The following question is typical:

Suppose a child visited at your house. Everybody played with the child and nobody played with you; what would you do?

a. Play by myself.
b. Cry.
c. Go away.
d. Hit the child.
e. I'd go to my mother.
f. Tell child to leave.
g. Make bid for attention.
h. Remarks—

From our original list of questions there were 33 which produced a response which was neither rare nor overwhelmingly prevalent and could therefore provide a basis for relatively reliable correlations. We shall refer to

each of these as the "selected" response to each question. Tetrachoric correlation coefficients were computed between the selected response from each question and the selected response from every other question. Each of these tetrachoric correlation coefficients indicates the degree to which the presence or absence of the selected response to one question was associated with the presence or absence of the selected response to another question. The resulting intercorrelations were recorded in a square table where each response was represented by a row of correlations and also by a column of correlations; the diagonal cells in the table were, of course, left empty. From such a square table, it was possible to compute the product-moment correlation coefficient between each column and every other column. (If there were k variables, each of the intercolumnar correlations would be based on k-2 of the variables. This is because of the empty diagonal positions.) Accordingly, each of these derived (intercolumnar) correlations could be entered in a comparable square table, so that it would be possible to make a point-by-point comparison between the original tetrachoric correlations (between the selected responses for the different questions) and the derived or intercolumnar correlations (i.e., the intercolumnar correlation computed between the set of correlation coefficients one of the responses has with all the other responses and the set of correlation coefficients the other response has with all the other responses).

Had our sample comprised persons who could give but one response to each question, we would have expected the derived correlations to provide an estimate of the original correlations, i.e., it is supposed that the nature of the relationship between two variables in an intercorrelated system could be inferable from the relationship each variable has with all the other variables in the system. (This reasoning concerning the use of context is analogous to the axiom "Things equal to the same thing are equal to each other." Nevertheless the cogency of this argument is not taken for granted and is subjected to some preliminary scrutiny in the following paragraphs.)

The use of this argument would lead one to expect that the correspondence between the original intercorrelations and the derived intercorrelations would be conspicuous and high. As a matter of fact, a high correlation coefficient between the original correlations and the derived correlations could be accepted as an evidence for the validity of our argument concerning the probable relationship between original and derived correlation coefficients. If a high correlation between original correlations and derived correlations were not found, we could suppose that either our argument concerning this relationship was unsound or that our sample comprised persons who could give alternate responses for most of the questions that were intercorrelated.

By an employment of the aforementioned data, we prepared a bivariate frequency distribution which indicated the similarity and dissimilarity between pairs of original and derived correlations. The original correlations were plotted on the abscissa; each of the original correlations was based on the responses of a sample of one hundred children and the points represented the magnitude of the intercorrelations between the various pairs possible among the 33 variables. The ordinate expressed the magnitude of the respective derived or intercolumnar correlations. Each of these correlations was based on 31 pairs of original correlations (in getting the intercolumnar correlations, two pairs are lost because of empty diagonals). Accordingly, each point in

our bivariate frequency distribution represented the respective magnitudes of the two correlations (original and derived) which had been determined for each possible pair of variables. From the trend indicated by this scatter diagram, it was evident that, in general, the original correlations and the derived correlations measure the same thing. The correlation coefficient between the original and derived correlations was .89. We could suppose that the lack of perfect correlation between the original and the derived values is due to three kinds of factors: one, the unreliability of the original correlations; two, discrepancies between the factoral composition of any pair of variables and the factoral representation comprising the context of variables; and three, the emergence of important derived correlations between some of the pairs of responses that, because of their alternative nature, produced original, direct correlations of but trivial magnitude.

We must have some evidence that the difference between the original correlations and the derived correlations is relevant to the problem which led us to a consideration of derived correlations. Specifically, we had anticipated that the alternative nature of the behavior for some pairs of variables would produce a direct correlation much smaller than the derived correlation (based on the functional similarity of the two variables) that is inferable from the total context of their responses to other variables. If this is a relevant and valid anticipation, we should expect the derived correlations to show more exceptionally large values (either positive or negative) than the observed correlations. An inspection of our plot supported the validity of our anticipation. In addition, we found that the variance of our set of derived correlations was reliably greater than the variance of our set of observed correlations.

Thus we receive some encouragement in our interest in derived correlations as a means of examining functional relationships between alternative responses. Specifically, (*a*) we take the evidence of relationship between derived and original correlations as an evidence that in general derived correlations have a descriptive significance which is similar to the original correlations; (*b*) we take the appearance of a greater number of extreme coefficients among our derived correlations as a probable (but not necessarily conclusive) indication that derived correlations may show relationships which are not shown in the original correlations.

Some Properties of Derived Correlations as Explored by Factor Analysis

Since our analysis of derived and original correlations involves the use of numerous intercorrelated variables, we may ask: "How may we expect the use of derived correlations to affect a factor analysis?" There are three ways in which such effects could be anticipated. These results are so much a consequence of our conception of the properties of the derived correlation that we may seek these anticipated effects on a factor analysis as a kind of validation for our general discussion of derived correlations. Specifically, we should expect the following results from a factor analysis based on derived correlations:

1. Since derived correlations are expected to yield more very large correlations than the original correlations, we should expect a solution involving derived correlations to involve more common factor variance, i.e., h^2 would be greater in a factor analysis of derived correlations than in a factor analysis of original correlations.

2. We would expect the original correlations (which could include misleadingly small correlations or possibly cor-

relations of a reversed sign from the derived correlations between functionally alternative responses) to produce a different number of factors from the derived correlations.

3. In addition, we should expect factors from the derived correlations to make sense and to be conceptually relevant to the factors from the original correlations. If no sense could be made of the factors from the derived correlations, or if they could not be related conceptually to the factors from the original correlations, one might suspect that the difference between the original and derived correlations was due to some accident of arithmetic or sampling, and that the difference did not have a relevance to the problem of behavior which interests us.

In order to test our anticipations concerning derived correlations and

TABLE 1*

ROTATED FACTOR LOADINGS BASED ON ORIGINAL CORRELATIONS

Responses	I	II	III	IV	V	VI	h^2
1. Dresses self	05	−04	*50*	03	−14	−06	28
2. Takes off his own coat	05	19	*58*	−02	−08	36	51
3. Cuts his own meat	05	21	*64*	08	03	−06	47
4. Looks for another friend if friend he is playing with leaves	*77*	−08	04	03	07	−03	61
5. Bandages himself when hurt	08	18	−14	42	−11	−11	26
6. Asks if he wants friend's toy	02	45	12	08	29	−21	35
7. Plays with own toys if another child won't let him play with his	*48*	−06	12	40	12	18	46
8. Plays alone if other children want to play with another child	09	−10	−20	25	*59*	16	49
9. Hits back if another child hits him on purpose	02	−37	−06	24	32	45	50
10. Tells an adult if another child takes his toy	−17	*93*	15	00	−22	07	97
11. Tells an adult if another child calls him a bad name	06	*71*	00	06	−25	12	59
12. Gives up his toy to another child if mother tells him to	17	05	−19	*70*	19	32	70
13. Doesn't play with another child if mother tells him not to	−02	21	−03	*73*	09	−03	59
14. Helps a child who has hurt himself	00	−46	19	*58*	−02	32	69
15. Avoids watching another child get spanked	01	18	−57	08	12	−23	43
16. Gets his ball out of garbage can himself	08	01	00	*−17*	26	50	35
17. Tells a little girl to pull down her dress	45	−23	16	17	*86*	−06	105
18. Tells a little boy to pull up his pants	36	−19	17	09	*67*	03	65
19. Tells an adult if another child wets his pants	06	*66*	−03	30	−02	−23	58
20. Replaces or fixes something of mother's that he breaks	*52*	13	09	06	12	33	42
21. Would wish for toys, pets, clothes if he could have a wish come true	02	*49*	16	−05	17	48	53

* The interpretation of the factors is based on positive loadings greater than .45.

their implications for factor analysis, a set of 21 responses was factor analyzed. (These 21 questions were selected from the larger group of 33 on the basis of their occurrence in both the present sample and another sample which we wish subsequently to study.) When the original tetrachoric intercorrelations provided by the 21 responses were factor analyzed, it was found that the common factor variance could be satisfactorily described by six centroid factors. When these factors were rotated orthogonally in order to reduce the complexity of the variables, a relatively definitive factor pattern emerged. Satisfactory meaning may be ascribed to at least five of these factors.

It will be seen from Table 1 that Factor I involves such responses as looking for another friend if the friend you are playing with leaves, playing with your own toys if another child won't let you play with his, and replacing or fixing something of your mother's if you break it. We are inclined to call this factor an indication of constructive reactions to social emergencies.

Factor II seems also to be plausible. It involves such responses as telling an adult if another child takes your toy, telling an adult if another child calls you a bad name, and telling an adult if another child wets his pants. This factor implies a dependency on others, particularly adults.

Factor III involves such responses as saying that you would dress yourself when you get up in the morning, that you would take off your own coat if you were too warm, and that you would cut your own meat when it was served to you. We have considered this factor as indicative of a kind of material or mechanical self-sufficiency.

Factor IV involves such verbal responses as saying that you would give up your toy to another child if your mother told you to, that you wouldn't play with another child if your mother told you not to, and that you would help a child if he hurt himself. This factor appears to imply a compliance with the expectations of others, particularly adults.

Factor V is an interesting factor, because it appears to imply a "proper" but possibly socially isolative type of response. It includes such items as saying that you would play alone if other children want to go play with another child, telling a little girl to pull down her dress if she pulls it up, and telling a little boy to pull up his pants.

According to our scheme for getting derived correlations, the square matrix of original intercorrelations was prepared and the correlations comprising each column were correlated with the correlations comprising every other column. The set of 21 variables which was the basis for the analysis described in Table 1 was reanalyzed, using the derived correlations. It is important to note that the style of the analysis for the derived correlations was identical with that for the original correlations, and that the centroid analyses were made by undergraduate assistants who were not aware of the nature of the data nor the purpose of the analyses. The results of the analysis of the derived correlations are presented in Table 2. The derived correlations yielded but four centroid factors, and, as before, these four centroid factors were rotated orthogonally in order to reduce the factorial complexity of the variables. The factors yielded by the analysis of the derived correlations are for the most part similar in their implications to the factors yielded by the original correlations. The most important difference concerns Factor V. Upon inspection it is apparent that the variables which defined Factor V in the analysis of original correlations are now primarily cor-

TABLE 2*

ROTATED FACTOR LOADINGS BASED ON DERIVED CORRELATIONS

Responses	I′	II′	III′	IV′	h^2
1. Dresses self	−07	03	*60*	−30	46
2. Takes off his own coat	−07	04	*82*	−25	74
3. Cuts his own meat	−14	20	*72*	00	58
4. Looks for another friend if friend he is playing with leaves	*76*	−26	−06	−10	66
5. Bandages himself when hurt	−10	17	−67	−10	50
6. Asks if he wants friend's toy	−03	*72*	11	25	59
7. Plays with own toys if another child won't let him play with his	*58*	−43	−16	13	56
8. Plays alone if other children want to play with another child	09	−29	−31	*72*	71
9. Hits back if another child hits him on purpose	25	−56	−03	39	65
10. Tells an adult if another child takes his toy	−30	*83*	13	−23	85
11. Tells an adult if another child calls him a bad name	−21	*86*	17	−04	81
12. Gives up his toy to another child if mother tells him to	15	−17	−31	*80*	79
13. Doesn't play with another child if mother tells him not to	−14	34	−28	*59*	56
14. Helps a child who has hurt himself	27	−51	21	*47*	61
15. Avoids watching another child get spanked	−03	18	−77	−02	63
16. Gets his ball out of garbage can himself	32	−20	41	23	36
17. Tells a little girl to pull down her dress	*71*	−35	−02	29	71
18. Tells a little boy to pull up his pants	*70*	−41	−03	28	74
19. Tells an adult if another child wets his pants	−03	*88*	−12	07	79
20. Replaces or fixes something of mother's that he breaks	*67*	−25	12	−21	57
21. Would wish for toys, pets, clothes if he could have a wish come true	30	*67*	27	17	64

* The interpretation of the factors is based on positive loadings greater than .45.

related with derived Factors I′ and IV′. Incidentally, it is also apparent that Factor VI in our analysis of the original correlations disappears, and the variables which are correlated with VI in Table 1 now appear in Table 2 to have important correlations with some of the other factors. Let us discuss each of the factors which emerges from our analysis of the derived correlations.

Factor I′ in Table 2 is somewhat modified from Factor I in Table 1. The nature of the modification is primarily a result of the fact that Factor I′ now bears conspicuous correlations with two of the variables which were important in Factor V in our analysis for the original correlations. Specifically, Factor I′ is still descriptive of the child's responses to questions which concern social emergencies, but in addition to the original set of constructive responses, it emphasizes the responses of telling a little girl to pull down her dress and telling a little boy to pull up his pants. We consider this modification

to imply a quality of independence of reaction in social emergencies (i.e., a child who discourages an exhibitionistic display by another child is showing an independent quality in his social reactions which was not implied by Factor I which emerged from the original correlations).

Factor II′ in Table 2 appears to be little different from Factor II in Table 1. In both the original and derived correlations this factor implies a dependency on others, particularly adults.

It may be noted also that Factor III′ in Table 2 is essentially the same as Factor III in Table 1.

Factor IV′ in Table 2 is much the same as Factor IV in Table 1, with one important exception. Playing alone when the other children want to play with another child contributed to a social isolation factor in our analysis of the original correlations in Table 1, whereas in our analysis of the derived correlations this response is highly correlated with Factor IV′ which, like Factor IV in Table 1, implies compliance with the expectations of others.

Let us review the respects in which we had expected an analysis of derived correlations to differ from an analysis of original correlations, and see if our data are in line with the expected trends, and if they are of such a nature as to contribute to the validity of our thoughts concerning the implications of alternative responses for correlational studies, particularly factor analysis.

1. We had said that derived correlations should in many instances be much larger than original correlations, and that accordingly the common factor variance would be greater for an analysis based on derived correlations than on the original correlations. This we have certainly found to be the case. For all the variables except four, the communalities in Table 2 are greater than the communalities shown in Table 1.

2. It was expected that the emergence of large derived correlations (indicative of relationships obscured in the original correlations because of the alternative nature of the response) should in general contribute to the appearance of a different number of factors in the derived correlations than in the original correlations. This we have certainly found to be the case. It is noteworthy that the analysis of the derived correlations produced fewer factors than the analysis of the original correlations, despite the fact that the derived factors involved more common factor variance than the original correlations.

3. We had expected the derived correlations to make sense and to be conceptually relevant to the original correlations. This we have also found to be the case in our data.

Conclusions

The use of original, direct correlations may be inappropriate either for drawing inferences concerning the functional or behavioral similarity of responses or for testing hypotheses concerning the functional or behavioral similarity of responses. This is particularly likely if there is a possibility that the responses or variables bear an *alternative* relationship with respect to each other.

In addition, it appears that the use of derived correlations may yield results which are different in their implication from results based on original, direct correlations. There is nothing in our analysis to indicate that such differences are irrelevant or primarily a result of sampling considerations. Instead, it appears that in data such as ours the use of derived correlations produces results which are not only different from those emerging from the analysis of original correlations but, from

the standpoint of comprehending the implications of our variables, are more economical than the use of original correlations.

It should be noted that the exact conditions under which reliance may be placed upon derived correlations are somewhat a function of the size of the sample, the reliability of the measures, the diversity of magnitude among the original intercorrelations (i.e., the contrasts provided by the context variables), and the number of responses or variables comprising the total context (i.e., the number of the variables comprising the columns of original intercorrelations). These matters will be the subject of a later report. From a primarily psychological standpoint, however, it is obvious also that the benefit which may result from the use of derived correlations may depend upon the nature of the context of variables in which we must infer or derive the relationship between the alternative responses. This problem of the context in which meaning may be inferred is primarily a psychological problem, and not different from the kind of problem that clinicians encounter continuously in their diagnostic and therapeutic work. In those investigations where one may lean upon a well-developed body of theory, the inclusion of suitable contextual variables or responses should be subject to deliberate, rational planning. Where no such theory or body of knowledge exists to guide us in choosing a relevant context, we must use hunch, intuition, etc. in selecting a context.

One cannot but wonder what changes would emerge if some of the factorial studies of personality were repeated with an employment of derived correlations where the context variables are merely all the variables (e.g., questionnaire responses, etc.) that the original studies comprised.

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